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ART. I.—*Observations on the epidemic Yellow Fever of Natchez, and of the South-west.* By JOHN W. MONETTE, M. D., of Washington, Mississippi.

(*Continued.*)

EPIDEMIC YELLOW FEVER AS IT PREVAILED IN THE SOUTHWEST IN THE SUMMER AND AUTUMN OF 1839.

9. *Alexandria.* This town is situated on the south bank of Red River, just below the rapids, and about 365 miles by the river from New Orleans, or a run of 48 hours by the steam packets. It is the head of steamboat navigation on the lower Red River, during low stages of water. The population generally amounts to about one thousand souls.

The health of this place had never been more uninterrupted in the summer season, than it was for three weeks after yellow fever had been epidemic in New Orleans. The regular steam packets made each their regular weekly trips to and from New Orleans. When the stage of the river admitted the passage

of a steamboat over the rapids, they ascended as far as Natchitoches.

The health of the bayou settlements south and east, at the distance of a few miles, was partially interrupted by a few cases of bilious fever. Yet Alexandria continued quite healthy until after nearly a dozen cases of yellow-fever had been introduced by steamboats from New Orleans; besides a number of other persons, who arrived in the meantime, from the same port, apparently in perfect health, and who were soon afterwards attacked by the same disease.

The weather about Alexandria, as well as in all the southwest, had been dry and very hot for weeks together. About the last of July the newspapers of that place complain that "the air at times was almost insupportable"; and that "the town was dry, dull, and healthy," with the mercury in the thermometer ranging from 84° to 92° daily. Such was the state of things in Alexandria when the epidemic was making its first advances in New Orleans; and in a few days more, the atmosphere would have been sufficiently infected to have generated an epidemic there, it being then the head of steamboat navigation from New Orleans, and the point of disembarkation for the swarms of Texian emigrants. An unusual rise or flood in Red River, at this critical juncture, postponed the epidemic for nearly six weeks. Red River continued in flood more or less until late in August, so as to admit the passage of steamboats for that length of time, over the rapids, as far as Natchitoches and farther. That place immediately became the principal destination of the steamboat trade; and Alexandria for the time was relieved of the swarms of emigrants daily ascending to upper Red River and to Texas, as well as of large quantities of freight and goods from the infected district of New Orleans.

So soon as the river subsided to a low stage again, and the rapids were impassable, Alexandria again became the head of steamboat navigation from New Orleans, about the last of August. It was a second time the depot for all the passengers, emigrants, and freight of every kind, shipped for the upper Red

River country. At this time, early in September, the epidemic in New Orleans was beginning to sweep off its scores every day; and every steamboat which arrived at Alexandria introduced more or less *fomites*, besides occasional cases of open yellow-fever, and many persons with the infection dormant in their systems. Cases of yellow fever immediately began to manifest themselves in that part of the town near the river, and in boarding-houses near the steamboat landings. By the 20th of September yellow fever was epidemic in the place; and it prevailed over the whole town before the first of October, although the majority of the people had fled to the country. So strongly infected was the local atmosphere of the place, that several persons from the country, who imprudently visited the town at that time, contracted the disease several days afterwards, and died in the country.

The disease was checked by the cold weather about the first of November, after *one hundred and five souls* had perished. Of these only six were females. The majority of all the deaths and cases occurred among emigrants, boatmen, or transient persons, principally from New Orleans.

Dr. Jackson of Alexandria, and other intelligent gentlemen, assured me that there was not a case of yellow fever in that place until it was introduced by the steamboats from New Orleans. Among the first cases were the engineer of the Houma and some of the crews attached to the Velocipede and Washington steam packets, which run regularly in that trade. The disease first spread from the common boarding houses, where the engineer and other hands of those packets had been left for medical attendance. Those who kept the houses were among the early subjects of the disease.

10. *Natchitoches.* The cases at this place were comparatively few, and were altogether in those who had arrived from New Orleans with the infection in their systems, or were open cases of fever discharged from the boats. These occurred also before the last of August, while the river admitted steamboats over the rapids, and while the epidemic in New Orleans had not reached its greatest degree of virulence. The fall in

the river, very opportunely, as the epidemic in New Orleans was approaching its most malignant grade, cut off further intercourse, and this preserved Natchitoches from a fatal epidemic. The Washington packet in her last trip left several persons sick of yellow fever at Natchitoches, who afterwards died. Natchitoches is 150 miles by the river above Alexandria, on the south-west bank of Red River, upon fine ancient alluvion, skirted by rolling pine uplands in the rear; population about 500.

I have not been able to learn that Natchitoches has ever been visited with epidemic yellow fever, as the rapids near Alexandria serve as an excellent natural quarantine ground, during the epidemics in New Orleans.

Having finished our notice of the river towns in which yellow fever prevailed in the autumn of 1839, we will proceed to note a few other points in Louisiana, upon the bayous south of Red River. In several towns of this region, likewise, yellow fever was introduced from New Orleans by steamboats. We desire the reader to bear in mind, that our steamers pass from New Orleans to the distance of 400 miles and return every week; and where the distance is much less, they perform their trips every three or four days. As yellow fever infection lays dormant in the system from three to nine days, before it manifests itself in open disease, persons may travel on steamboats to the distance of 300 and even 500 miles, in perfect health, after they have contracted the disease. If the distance is less they may arrive at their destination several days before the disease developes itself in their systems. There is no part of the southwestern settlements of Louisiana which is more than 250 miles from New Orleans, by the steamboat route; of course the trip to and from the city is performed every week at most, when the business is profitable. At the outbreak of an epidemic in the city thousands of persons leave it within the first three weeks, and disperse through the numerous inland towns within one hundred and fifty miles direct from the city. These points they reach on steamboats in 24 or 36 hours. Under the excitement of anx-

iety, alarm, and apprehension, the equable condition of the system is disturbed, and many are attacked soon after they reach their destined asylum. Those towns to which many persons, with their families, clothing, bedding, and the like, resort, are greatly exposed to the danger of an epidemic; and especially if the condition of the general atmosphere be favorable to its dissemination. In this manner the yellow-fever was introduced into the following towns in the autumn of 1839:—viz., Thibedeauxville, Franklin, New Iberia, St. Martinsville, and Oppelousas.

1. *Thibedeauxville* is situated on the south side of the Lafourche, about forty miles from its efflux at Donaldsonville, and 125 miles, by steamboat route, from New Orleans. The population is about 300 souls, chiefly Creole French, and some Americans, and transient persons. In low stages of the Mississippi, steamboats cannot pass from the river into the Lafourche, on account of the shoal bottom for several miles of its upper course. At such times the small steamboats which remain in the Lafourche, run regularly from the upper to the lower settlements, a distance of about 100 miles. Donaldsonville then is the depot for the freight and passengers from New Orleans for the Lafourche, and is connected with the Lafourche line of boats by a portage of a few miles. Thibedeauxville is the central town for these settlements, and a regular landing point for steamboats.

In this place the population enjoyed uninterrupted health, in the fall of 1839, until many persons arrived from New Orleans, towards the last of August, for retirement from the epidemic. The *first five or six cases of yellow fever* were unquestionably in persons recently from that city; and none pretend to question the fact, that these cases were introduced from New Orleans. Cases occurred subsequently during the month of September, until the whole number was about *twenty-five*. Some of the latter could not be traced to New Orleans, but to infection or *fomites* introduced from that city. The disease did not prevail as an epidemic, for the greater number of cases were contracted in New Orleans.

Mr. W. B. Shields, an intelligent planter in that region, assures me of these facts. The number of deaths in this town was about fifteen.

2. *Franklin.* This town, like the last, is situated on high alluvion, on the Teche, at the head of steamboat navigation in low stages of the river. The distance from New Orleans by the steamboat route, is about 250 miles. There is also a more direct route in high stages of the river. The trade by steamboats with New Orleans continues all the year. Population 250.

This town, like all the interior towns, was uncommonly healthy during the summer until after yellow fever had become epidemic in New Orleans, and many persons flying from that disease, had arrived from the city about the first of September. In addition to which, in the first week of September, a steamboat arrived from New Orleans with many persons on board, several of whom were attacked soon afterwards with yellow fever, besides *two cases* which developed themselves on the way, one of which died before the boat reached Franklin. This boat proved to be infected; for several persons died who had not been exposed to any other source of infection, and who were attacked with yellow fever a few days after having made a visit to this boat. The clerk of the Parish court was one of them. In less than a week after this boat arrived at the landing, several persons in that immediate vicinity took the disease and also died. The disease was considered epidemic after the 15th of September, and did not cease until checked by frost early in November. The number of deaths in the village and vicinity, was about 25; the whole number of cases about 45.

3. *New Iberia.* This town, like the last, is situated on the Teche, and upon high ancient alluvion, about 30 miles above Franklin, and at the head of low tide-water. The population is about 160.

This village continued very healthy until about the 10th of September, when cases of yellow fever began to present themselves in the persons of those who had recently arrived from New

Orleans, by way of the Plaquemines. Such were the first cases of yellow fever in New Iberia, in 1839. I am not apprised that the disease was ever there before.

Soon after the first cases of this kind, the disease began to spread among those of the place who had been exposed to no other source of infection than the steamboats, the sick, and the *fomites* imported in those boats. The whole number of *deaths*, in and near this town, was about *twenty*. For the facts I am indebted to the Hon. B. G. Tenney, and other intelligent men.

4. *St. Martinsville* is also on the Teche, and about 80 miles by the bayou above Franklin. This village has been noted for its general salubrity, and yellow fever was unknown there until 1839. Several cases occurred, as was the case at Natchitoches, in persons who had arrived from New Orleans; but being in a great measure cut off from a continued supply of cases and *fomites*, by low water, it did not become epidemic in this village.

5. *Oppelousas* is a small, scattered village, situated in a rolling prairie, proverbial for health, and remote from all those causes which are said to generate yellow-fever miasm. It is remote from any navigable stream, and the nearest steamboat landing is nearly six miles distant. With this point, however, the village has direct and constant intercourse by a regular line of daily stages; and thus a direct communication is kept up from New Orleans to Oppelousas. The route between the two points is made in less than 48 hours: a boat may leave New Orleans, loaded with freight and passengers, together with yellow-fever *fomites*, and in 48 hours one half of the whole cargo, and all the passengers, may be in Oppelousas. The intercourse is not discontinued during the whole year; and previous to the experience of 1839, it was supposed, even by those who were well convinced of the importable nature of the disease, that the interruption in the route by a land portage of six miles, was a sufficient guaranty of safety.

This village has long been a resort for many of the inhabitants of New Orleans, when compelled by the epidemic to re-

tire from the city; and in no case have I been able to learn that yellow fever has been ever communicated in its epidemic form, to the inhabitants, until the autumn of 1839. This year the health of the place was uninterrupted, until the first of September, when cases of this disease began to manifest themselves in the persons of those who had recently arrived from New Orleans. For ten days it was confined exclusively to the people of New Orleans, and those recently returned from that city, with whom the village was thronged. Cases multiplied daily, and by the middle of September it was considered as epidemic, when most of the people deserted the place. The disease was epidemic until November, when the number of deaths had increased to *forty-seven*, of whom seventeen were natives of the place.

An infected atmosphere was created, and *first*, especially in some houses where cases had occurred. Some persons from the country, in perfect health, contracted the disease and died after having made a short visit to those houses, supposed to be infected.

Nor was the dissemination of yellow fever from New Orleans, restricted to the inland towns of Louisiana and Mississippi. It was carried likewise to Texas. The principal port of Texas is Galveston, about 500 miles by the sea route from New Orleans. This route is made by the regular packet steamboats in less than 60 hours. Thus, in two days and a half, a steamboat loaded with freight and passengers from the infected port of New Orleans, will be discharging the same at the wharves of Galveston. This kind of intercourse is almost daily during the whole business season. From Galveston to Houston is a drive of eight hours by the stage; and the intercourse is direct and uninterrupted. The port of Galveston has little or no direct trade with any foreign port except New Orleans—it is situated upon an island of sand and of course not in a miasmatic atmosphere, according to the common doctrine. Yet it did become infected with yellow fever as an epidemic, and many lives were lost. Others died with yellow fever at Houston, who left New Orleans only 8

or 10 days before. Galveston received the seeds of the disease from New Orleans, and from New Orleans alone. I am well aware that the Galveston epidemic has been ascribed to the magic influence of marsh miasm, city filth, and impure wharves. It began about the wharves and shipping, we admit, as it does at every port.

If we examine into the circumstances of its prevalence on the northern coast of the Mexican Gulf, from New Orleans eastward to St. Augustine, and even to Charleston, we shall find abundant reason to infer that in the more important ports, yellow fever was introduced from Havana, Vera Cruz and other ports in the West Indies; while in the smaller trading towns it was introduced from New Orleans, Mobile and Charleston. All the commercial ports suffered from the disease in direct proportion to the amount of direct trade with the West Indies, and the smaller towns suffered in due proportion to the direct intercourse with these infected ports of the United States. Other towns, whose latitude and situation, according to the common views of the yellow-fever miasm, would be most exposed to this disease, were entirely free from it, when their intercourse was cut off with the infected ports.

1. *Bay of St. Louis.* This is a small village situated upon the bay of that name, about 60 miles east of New Orleans. This village is entirely without commerce or intercourse with any foreign port, and is made up chiefly of cottages and villas occupied by wealthy families from New Orleans in time of epidemic yellow fever, and at other times as a pleasant summer retreat from the heat and crowded population of the city. The site was chosen for this purpose as one peculiarly free from all the usual sources of disease of all kinds. It is upon a flat sand beach remote from marsh vapors and exhalation, and has always been considered as remarkable for health, and as such, has been a frequent resort for invalids from the interior of Louisiana and Mississippi. The cottages and villas are extremely neat and airy, spread chiefly in one range of buildings at the distance of nearly fifty yards from

each other. Besides these there is an extensive hotel for the accommodation of those who have no permanent provision for a residence. There is one steamboat wharf, but no trade or commercial intercourse with any port except New Orleans, and of course none of the usual accumulations of city filth.

Yet when this village has been much frequented and thronged by individuals and families from New Orleans, with constant intercourse by steamboats with the city during the epidemic, it has been repeatedly visited also with yellow-fever cases; and occasionally an infected atmosphere has been generated in one or more houses, from which the disease has been occasionally propagated to those who had been exposed to no other source of infection; and the people have been compelled once or twice to desert the place for safety. The first cases of yellow fever at any time occurring here, have been unquestionably those who had retired from the epidemic in New Orleans or Mobile. In 1839 there was a number of such cases here; but an infected atmosphere was not generated which would produce the disease in those who had been free from any other exposure. What local accumulations of filth, or what marshes are here to generate yellow-fever miasm?

2. *The Bay of Biloxi* is about 30 miles further east, and about 50 miles west of Mobile Bay. It is situated upon the margin of the sea, and like the last, is formed chiefly by about 30 cottages and elegant villas, strung along the beach for several hundred yards. This place is one of common retreat for the two cities of Mobile and New Orleans during their epidemic visitations. Scarcely a season passes, in epidemic years, without several cases of yellow fever, developed in those who arrived apparently in perfect health from one of these infected cities. Yet, like St. Louis, it has been mostly free from an epidemic form of disease. The simple reason for this is, that the local atmosphere in these places is not sufficiently contaminated by a crowded population to form a suitable nidus for the reception of the infection or *fomites*. These facts are given upon the authority of G. L. C. Davis, Esq., an old resident of New Orleans familiar with these pla-

ces, and it is corroborated by the authority of hundreds. In 1839 seven cases of yellow fever occurred at Belman's Hotel near the steamboat wharf, and the local atmosphere of the Hotel became infected, as was evident to all interested. Several persons contracted the disease from that house.

3. *Mobile City.* This city has an extensive commercial intercourse with the West Indies and other parts of the world. The direct intercourse between Mobile and tropical ports is no more restricted than that of New Orleans, except in point of commercial importance. Besides this it has constant intercourse by steamboats and sea vessels with New Orleans. Owing to its own direct trade with the West Indies, it becomes infected with yellow fever almost simultaneously with New Orleans, while those towns which derive their infection from either, or both, are not visited by the disease epidemically for several weeks after it has prevailed in one or both of these cities. The disease in Mobile is no less fatal than it is in New Orleans. The average mortality in both is at least *one-third* of the whole number of cases; whereas in inland towns the mortality seldom falls short of *one half*, notwithstanding the assertions of some men to the contrary.

The disease in 1839 made its appearance among the shipping at Mobile, simultaneously with the same disease in New Orleans. In both places, the first cases were among the shipping exclusively, or among those who had frequented certain vessels. At Mobile the disease was advancing *pari passu* with that in New Orleans until the 20th of August, when a south wind sprung up for several days, and the disease, for a while, appeared to be arrested. But by the *third* of September it began to spread with great virulence, and extended to the inhabitants of the city. It continued to spread among them for nearly six weeks, with great mortality, until the 20th of October, when it began to abate after a strong wind and change of weather. On the first of November the disease was considered extinct. The total number of deaths was about 650—of these, 147 died in August, 383 in September, and 120 in October. On the first and second days of September there were 22 deaths; and 127 in the *next seven* days.

4. *Pensacola* is situated upon a bay of the same name, about 20 miles east of Mobile, and nearly in the same latitude; but having no West India commerce, it escaped yellow fever. Although this town had suffered severely in former years, from epidemic yellow fever, yet in 1839, when every commercial port was ravaged by that pestilence, Pensacola escaped. Why was Pensacola exempt? Its population was far more numerous than many towns of Louisiana which suffered severely; it is in the same latitude of towns infected, and as far south as Mobile. Why then did it escape, if yellow fever be endemic on this coast? We answer, that Pensacola escaped yellow fever this year, simply because it was not introduced there as it had been in other towns and ports. Pensacola was not crowded with persons who had already imbibed the infection; the town, the stores, and houses, were not filled with goods, clothing, beds and the like from infected towns or ports; it did not carry on a free and extensive commerce with infected ports of the West Indies, with arrivals almost daily of ship-loads of infected air from Havana and Vera Cruz. The streets were not thronged with raw Europeans or unacclimated strangers. These are the negative causes of her exemption.

Pensacola is a naval station, and during the summer and autumn of 1839, the harbor contained a large number of vessels of war, with their complement of sailors and marines. During the greater part of this time, there were at least 30 vessels of war, with their aggregate complements of 3500 men, besides the ordinary population of the place, giving an aggregate of at least 5000 souls.* Yet on the 31st of August, at the very time when yellow fever was ravaging New Orleans and Mobile, the whole population of Pensacola, including the fleets, was perfectly healthy.

During the epidemic season the United States squadron lost five men by yellow fever, having contracted the infection before entering the harbor. The French frigate La Gloiré, en-

*See Pensacola Gazette, Aug. 31.

tered the harbor with twenty cases of yellow fever on board. Due precaution was taken to prevent intercourse with the population of the town; the frigate was anchored out in the bay, and the sick were carried to the Navy Hospital, for medical aid. The disease was circumvented and it disappeared.

The French frigate had not become infected, as the air on board had not been contaminated with the infected air of the West India ports, by lying for weeks at the wharves, as merchant vessels do; the men had contracted the disease by going ashore at such places, and the vessel was prevented from being infected by the early removal of the sick to the naval hospital. The disease did not spread at the hospital, because, as we have before remarked, the air in a hospital is not prepared for the dissemination of the disease, like the air of a crowded town or city. The restricted intercourse between the population and the naval forces, prevented the extension of the disease in the town.

The first epidemic in Pensacola, after it passed into the possession of the United States, was in the summer of 1822, or one year after it had ravaged St. Augustine. Up to that period it had been remarkable as a place of extraordinary salubrity. But immediately after the influx of northern emigrants, a trade sprung up between this town and the West Indies, and the yellow fever was introduced. Mr. Williams narrates the facts, and after yielding to the vulgar prejudice, in admitting that it was partly caused by filth and putrescent matters near the harbor, he proceeds to say: "*the pestilence spread like wild-fire*, immediately after the distribution among the huckster shops of the cargo of spoiled fish which *arrived from Cuba*, sweeping whole families, and often whole streets, in one general destruction." The mortality, he informs us, was altogether among the recent population; "for *none of the old inhabitants* were afflicted with the pestilence."*

Here it would appear evident that the active poison which produced the epidemic was certainly brought *from Cuba*, even

* Williams' Florida, pp. 15, 16.

if it were in the "*spoiled codfish.*" Like most other cases of a similar nature, it is likely that the codfish were distributed, sold and eaten, without any discovery of their spoiled condition, until after the close of the epidemic, when probably this plausible conclusion was arrived at. Every shop-keeper who supplied himself with these fish, doubtless *visited the vessel*, and contracted the disease in person, from the infected vessel.

How was it in 1825? Pensacola was thronged with northern adventurers, with trading vessels, and numerous importations for the new settlements, and yellow fever was introduced and prevailed with most destructive mortality. Hence there is nothing in the situation or location of Pensacola, which renders it more exempt from, or more liable to yellow fever, than any other town, under the same contingent circumstances.

The *first* epidemic yellow fever on record at Pensacola, was in 1765, at the close of the French and Spanish war, immediately after a British garrison had taken possession of the province of Florida. "A regiment of soldiers was sent from England to Pensacola, during a very hot summer. On their arrival they were confined day and night within the walls of the fort at Barrancas, which excluded the sea-breeze. They soon became infected with malignant fever, which proved very fatal to the common soldiers."* This epidemic was thus introduced by way of the West Indies, where all the troops of Great Britain were obliged to pass on their destined route. These troops, chiefly fresh from England, with their European constitutions and habits, were brought through the West Indies to Florida, at the very time when yellow fever was epidemic in those seas. They were quartered in a confined fort during the most oppressive heats of summer, under all the circumstances calculated to generate infectious air. This was soon accomplished; and the introduction of supplies of clothing, arms, &c., from the West Indies, furnished them with the active cause of disease, if they did not bring it with them on their first arrival. Those troops in the port, which had no commu-

* Williams' Florida, p. 15.

nication with the garrison in the Barrancas, continued to enjoy perfect health.

Let us inquire into a few other points along the northern shore of the Mexican Gulf. We find Mobile, a commercial city, desolated with yellow fever. At the next bay only 60 miles distant, Pensacola, *not a trading town*, but having at least 3500 men in the harbor, besides her own population, entirely escapes disease; and although twenty-five cases were introduced into the open suburbs of the town, no epidemic supervenes. A little further east, we find the little port of *St. Joseph's*, where a lively trade had sprung up a year or two previously for the new settlements on the lower Apalachicola, with direct intercourse with New Orleans and Mobile, as well as with the West Indies, ravaged by the same pestilence which was then prevailing in those ports. A little further east and south, on the western side of the peninsula of Florida, is Tampa Bay. This point, also, within the last two years had attracted a smart trading intercourse from the same ports, and after the Seminoles had been driven back, was thronged with settlers and adventurers. This place too became infected, and many of its inhabitants died. Other points were visited in the same manner.

St. Augustine, on the eastern side of East Florida, had been proverbial for health during the Spanish regimé, and was never visited with yellow fever until it came under the jurisdiction of the United States. But in the autumn of 1821, having become thronged with northern emigrants and unacclimated adventurers (and mainly on account of its proverbial salubrity), it was suddenly visited by a most fatal epidemic, which swept off a large proportion of all who were attacked.

In 1839, the population of this town was quite healthy until about the middle of August, when yellow fever of a very mild character prevailed to a considerable extent. The place labored under this epidemic from the middle of August until the 12th of November. During this time, out of *seven hundred cases*, only about *fifty* died—or *one* in *fourteen!!* Most cases yielded readily to a judicious mode of treatment. *Yellow fever* never has prevailed epidemically in the United

States, I am persuaded, where there were *more than two recoveries to one death*, including all the genuine cases of yellow fever.

In 1839, I am strongly inclined to think, that St. Augustine was visited with epidemic *bilious* fever, besides a few cases, comparatively, of yellow fever, which may have been communicated from the shipping to those who visited their holds and cabins.

In regard to St. Augustine, the *first epidemic yellow fever* occurred under circumstances similar to those which attended the first epidemic in the Barrancas at Pensacola, viz.: the change of regimé. In the latter it broke out upon the occupancy of the place by British troops by way of the West Indies; in the former, or St. Augustiue, it broke out soon after the place became occupied by the United States. Mr. Williams * states that "in 1821 St. Augustine was visited with yellow fever. It broke out in several buildings situated in the back part of the city, which had been for a long time closed up, their owners having retired to Havana. On the cession of the country to the United States, a *sudden increase of population* caused these houses to be thrown open and *rented to strangers*. One of them was rented to several American officers, and three of them fell immediate victims to the fatal disease. *In some cases the sickness commenced in vessels lying in the harbor*, which had brought fruit *from Cuba*. *One of these, on the voyage, had lost the captain and most of the crew by sickness. Some early cases of fever were traced to other vessels.*..... Since that period St. Augustine has been distinguished as one of *the most healthy spots in the United States*." This is a simple, unvarnished narrative, unswayed by prejudice or theory, giving the facts without comment.

6. *Savannah*, the principal commercial port of Georgia, is liable to frequent visitations of epidemic yellow fever. In 1839, the population of the city was uncommonly healthy for at least three weeks after cases of yellow fever began to appear.

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* Williams' Florida.

We do not design to trace the origin and spread of the epidemic in Savannah, but merely to note the fact, and refer to the circumstance of its appearance among the vessels in the port, several weeks before it became epidemic in the city. The intelligent reader who has perused the previous pages, on the importable character of this disease, can make his own inferences. It first spread from the shipping to the population near the wharves.

7. *Charleston, S. C.*, has been an important commercial port from the earliest settlement under the British Crown. The first epidemic yellow fever in this place, was in the autumn of 1700. It likewise prevailed again in the years 1732, '39, '45, '48, and '92. In more recent times it has been visited occasionally with this epidemic, in its most malignant form. And it is clearly proven by the records of the times, that there have been occasionally cases among the vessels in port, when no epidemic supervened.

We propose briefly to examine the circumstances connected with the prevalence of yellow fever in this city in the fall of 1839, and its *subsequent appearance* at Augusta in the interior of Georgia.

The newspapers represent Charleston as having been remarkably healthy until the first of July. A few cases of yellow fever had occurred among the shipping in port soon after the middle of June; and cases from the vessels were received into the marine hospital on the 25th and 26th of June,* at which time cases were frequent among the shipping. The Charleston Courier, of the 24th of June, 1839, says up to that time, "the cases of yellow fever were few in number, and *confined exclusively to a few vessels* in our harbor, and to *seamen on board those vessels;*" again, "*not a single case had occurred on shore.*" The same paper again declares, that, up to the 24th of June (1839), "among the vast number of *persons now in our city*, liable to yellow fever, *not a landsman has been*

* See Dr. Strobel's Report as Physician to the Marine Hospital. See also the Charleston Courier and New Orleans Bulletin.

attacked by it." The number of cases began to increase daily among the shipping, and chiefly among the crews who were unacclimated. During this time the papers of Charleston continued to repudiate the doctrine which admits the possibility of yellow fever being in any case contagious, or in any wise communicable from the sick to the healthy population; they endeavored to calm the fears of the people against all possible danger, as it possessed, in their opinion, no more communicable properties than bilious or intermittent fever. In this manner they made themselves responsible for the death of hundreds. Many, relying upon such advisers, neglected the necessary precautions; intercourse with the infected vessels was indirectly encouraged, and by this means it was soon disseminated among the contiguous population; and in a few days more it was epidemic among the people residing near the wharves adjacent the infected vessels.*

On the 10th of July the Charleston Mercury urged in very strenuous terms the necessity for "captains and owners of vessels" to be prompt in procuring medical aid, in the *first* attack, while the mass of the citizens were, in effect, encouraged to introduce the disease into the city. The laboring classes as usual, and those who carry on the commercial transactions with the shipping, were the first victims among the inhabitants. The disease continued to spread slowly from the shipping to the population adjacent to the wharves, until the last of July, when it was admitted to be epidemic in the city.†

It is not my purpose in this or any other case, to point out the particular vessel which introduced the disease, because I believe that many are concerned in its introduction; it is the work of some days or weeks, and each vessel from an infected port, has its special agency in inducing an epidemic. There were vessels in the port of Charleston, with yellow fever on board, as early as the 7th of June. The brig Burmah

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* See New Orleans Bulletin, Aug. 10, 1839, quoted from N. Y. Express.

† Charleston Mercury.

is the first that we can refer to; the brig Braganza was soon after there with yellow fever on board, and many others subsequently.

All agree in the fact that for fifteen days after the disease began to excite alarm, it was confined exclusively to the shipping, and such as came from West India ports. Yet the public press, without evil design, we are sure, endeavored to bolster up the belief that it was a local disease, and a mere grade of bilious fever, produced by local atmospheric influences, although the resident population was at first exempt; while those who were least exposed to such influences, if they had existed, were the only victims. But blinded by an interested prejudice, they would not see.

As soon as the people perceived that the pestilence was extending the sphere of its action, they immediately adopted the usual and only measure for safety, and fled to the country. The rail-road to Augusta offered the most convenient and direct conveyance, and accordingly many embraced the opportunity of retiring to that high and healthy interior town, in the State of Georgia, *where yellow fever had never been known.* Some took with them beds, bedding, goods, and the articles usually taken by retreating families and merchants.

The disease was epidemic in Charleston from the first of August until the 20th of October, when it began to decline in virulence. The number of deaths I have not been able to procure.

8. *Augusta, Ga.* This city, within a few years, has become an important *commercial* depot for importations made through Charleston and Savannah for an extensive region of country in the interior of Georgia and South Carolina. It is also the point at which a great portion of the interior cotton crop of these two States is collected for those two maritime ports. As such depot it is the head of steamboat navigation on the Savannah river; and in 1839 was the *terminus* of the great Charleston rail-road. It is situated upon the Savannah river, 230 miles above the port of Savannah, and 120 miles by the rail-road from Charleston. The site is chiefly tertiary up-

land formation, in a high and healthy region, beyond the influence of marsh miasm, and all those matters to which yellow fever epidemics in maritime ports are ascribed.

Previously to the summer of 1839, it was proverbial for its salubrity, and noted for its *exemption* from yellow fever during all the ravages of that disease in Charleston and Savannah. Such had been its exemption from this disease at such times, that it furnished a plausible argument against the probable *importability* of the disease; for if importable at all, it might have been at some time transported from Charleston or Savannah, with which constant intercourse was maintained. As such, the fact was used by professor Dickinson of Charleston, in his remarks upon the *yellow fever* of that city in 1838. When those remarks were penned early in the summer of 1839, little did the learned professor think that nature herself would so signally confute the argument and vindicate truth and the immutable laws of nature; that even before his "remarks" met the public eye, she would snatch from him the argument which had been fallaciously used by many others in a similar manner.* But like a philosopher and a votary of scientific truth, he received the lesson taught by nature and renounced his error.†

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*See Med. Journal published by John Bell, M.D., Philadelphia, December 1839.

†In a letter to Dr. Strobel, of Charleston, dated January 14th, 1840, and published in the Charleston papers, and re-published in the New Orleans Morning Advertiser of December, 1841, Professor Dickinson acknowledges his change of opinion relative to the *importable* nature of yellow fever infection; viz—"I believe yellow fever to be *transmissible or communicable from one city to another*, provided the general circumstances of the two are similar or analogous; that is, I believe the unknown and *obscure cause of yellow fever is transportable from place to place, and in a variety of modes.* This cause requires for its efficiency an undefined occurrence of favorable circumstances, without which it will fail to produce its specific influences; but this is true of the agency of every cause of disease.

Of its contagiousness (using the word in its limited and popular sense), its direct propagation from one subject to another, I have never witnessed an example, and *until recently* should have denied the possession of this property in our climate. *The events of the last summer,*

Augusta remained uncommonly healthy for fifteen days after yellow fever had been epidemic in Charleston, and for nearly ten days after it had been epidemic also in Savannah. Within a few days after the general alarm and flight from Charleston, cases began to appear in Augusta, and chiefly in those who had retired from those infected points, until about the 15th of August. On the 17th and 18th of August, cases began to multiply, and on the 20th it was considered epidemic. From the 18th to the 31st of August, the deaths were only twenty-eight. From that time until the last of September the disease raged with great mortality, after which it began to abate. The number of deaths in the month of September was reported at one hundred and seventy.

The history of this epidemic will show that Augusta was exposed to a double source of infection; first, by the rail-road from Charleston, upon which the train of cars arrive in eight or ten hours; and secondly, by steamboats which reach Augusta in thirty-six hours from Savannah. The former makes daily trips, and the latter are successively arriving once or twice a week, with every variety of articles which retain the infected air in them.

The continued arrivals upon the cars, of persons with the latent seeds of yellow fever in their systems, to be soon developed in open yellow fever, besides other modes of introducing infection, tended strongly to accelerate the epidemic constitution of the city air for leading on the epidemic.

As soon as the inhabitants of Augusta were convinced that they were invaded by this unwelcome visitation, they immediately fled from the city and took refuge in the surrounding country. The physicians of the place, who believed in the local origin of yellow fever epidemics, as usual, began to search

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however, have inclined me to entertain an *opposite opinion*. An impartial perusal of the statements and arguments of Pym, Blane, Arejula, Wistar, Hosack and Monette, has satisfied me that it deserves to be ranked among contagious diseases.

I am sir, respectfully, your obedient sev't.

B. B. Strobel, M. D. Saml. Henry Dickinson "

for the usual imaginary sources of the miasm. It would havebeen strange if they had not been able to discover some innocent collection of animal or vegetable matter, which would serve, with the credulous, as a scape-goat to bear off the sins of the commercial members of the community who had been instrumental in its introduction, while it diverted the attention of the intelligent from the true cause of the disease. Accordingly the press soon announced that they had discovered "a pile of putrescent vegetable and animal matters near the upper trash-wharf," which no doubt had at once caused all the mischief, although it had remained there in a harmless state since the first of June!

Why did not yellow fever manifest itself in Augusta at the same time it made its appearance in Charleston and Savannah? Why was the epidemic in Augusta deferred until Charleston and Savannah were almost deserted by their inhabitants and the transient population? Why did it not commence in Augusta first, if it proceeded from local causes? Did the arrival of the people from those infected cities excite the extrication of pestiferous miasm from local causes, which had laid dormant up to that time?

We have shown already that an infected air is often generated or imported in vessels arriving in our ports from the West Indies, and that persons have beyond doubt, contracted yellow fever by visiting such vessels after they had arrived. The same atmosphere is likewise generated in steamboats from infected towns. Cholera has in like manner been introduced into many towns within the valley of the Mississippi, as I have abundant evidence to prove, during and subsequent to the Black Hawk war, on the upper Mississippi, several years ago. A cholera atmosphere may be generated from a number of cases on any steamboat or vessel, or in any house or town. The same is true of yellow fever under favorable circumstances in the local atmosphere of any given town.

Professor Dickinson, before cited, observes: "*Cholera is importable.* It may be conveyed from one city to another *by a vessel*; or it may originate during an unfortunate voyage on

board a vessel, which shall not only disease her passengers, but will infect persons who may venture on board in perfect health, after her arrival in port. In either case, the vessel herself, or the atmosphere which she contains, shall be in a state capable of communicating the disease to those who visit her. Cholera is, therefore, clearly a proper subject for quarantine regulations, and these should be strict and perfectly effectual."*

This is precisely the case with yellow fever during the hot and oppressive weather of summer in the southern part of the United States. The whole weight of impartial testimony sustains the position; and as it has been known to produce the most malignant and terrific epidemics in the ports and towns of the United States, we deem it a most important and proper subject of quarantine regulations for the protection of our citizens.

This point we shall endeavor still further to sustain and elucidate by facts and reasoning, which we desire to submit to the impartial reader and the candid enquirer after truth.

ORDINARY MODE OF INTRODUCING INFECTION.

The manner in which infection is introduced into towns and cities on the lower Mississippi, is through the medium or agency of ships or steamboats, as we have already shown in the early portion of our remarks. New Orleans is the point from which it is communicated to the interior river towns; and this city itself, in like manner, is annually supplied with a due portion of *fomites*, and infected air from West India ports, imported chiefly in trading vessels, which arrive almost daily from all those ports where yellow fever is known to be endemic at nearly all seasons of the year.

When any town or city, and especially those south of lat. 35°, contains *infectious air*, as already illustrated, that place is in a *proper condition* for the dissemination of *yellow-fever infection*, and for the prevalence of a fatal epidemic, although

*See Am. Jour. Med. Sciences, 1833, vol. 13, p. 359-60.

the population may have been entirely healthy immediately previous to the introduction of such infection. If this condition of the local atmosphere exists in an eminent degree, a smaller amount of infected air, or *fomites*, will generate the epidemic; if this condition of the local atmosphere is less concentrated, a larger amount of infection or fomites is requisite to produce the same effect; and a town or city in the latter condition may escape an epidemic, after the introduction of an amount of infection, which, under other circumstances, would have generated an epidemic. This position we desire to be borne in mind, for it will prevent the dissenting reader from urging those objections which are nugatory to one who carefully weighs this point.

Hence the arrival of infected ships, and steamboats, with goods on board impregnated with infection, in any port or town, may be eminently dangerous, or it may be comparatively harmless, according to the circumstances of the case. The number of steamboats on the waters of the Mississippi trading to New Orleans is not less than *three hundred.*

All who are practically conversant with yellow fever in the southern States, know full well, that beds, blankets, clothing, and all other porous articles, which remain a short time in a strongly infected atmosphere, whether limited to a single room, house, or ship, or to the mercantile part of a city, become impregnated with the active *materies morbi*, and will produce the disease in those who are subsequently exposed to its operation. It is equally well known that the entire atmosphere in a room will become so thoroughly infected, under certain circumstances, as to produce the disease in divers individuals who may enter such room, and breathe its atmosphere even for a short time.

Let us suppose a case. Yellow fever is epidemic in New Orleans; it begins to carry off its victims at the rate of 50 or 60 per day. Regardless of the danger, hundreds of strangers remain in the city, vainly hoping to escape the disease; one ship after another, crowded with foreign emigrants, German or Irish, arrive in the desolated city, and take up a habitation

in some refuse or desolate houses and sheds. Lured by the high wages for labor, they remain and expose themselves in the infected district, until the disease spreads rapidly among them. Every day diminishes their numbers, and a new focus of infection is generated in their camp or lodgement. Finding that the pestilence increases its ravages daily, without a prospect of abatement, they determine to escape from the city and pass on into the interior. They engage passage on the first steamboat for St. Louis or Louisville. The boat has been lying at the wharf in the infected district for several days, taking in freight of every description, and all more or less impregnated with infection. At length the boat is ready for her departure, and our foreign emigrants are crowded into the lower deck to the number of 50 or 100 souls, men, women and children, a motley crowd, packed and stowed, with their filthy bedding and clothes, into every recess and corner of the boat. They continue to sicken and die in this crowded atmosphere, during the hot sultry weather of August or September; and occasionally the boat lands to take in wood, and to bury two or three deck passengers who may have died the last night. The whole atmosphere in that part of the boat, and every article of bedding, clothing, and the like, becomes thoroughly infected, and will produce the disease in those who visit the boat at the different landings on her way up the river. By the time the boat reaches St. Louis or the mouth of the Ohio, fifteen or twenty of this miserable crowd have been laid in quiet repose along the solitary banks of the Mississippi.

This is not a case entirely of fancy; the parallel complete has been too often witnessed on the Mississippi within the last 20 years.

In many cases boats in this condition, on their ascending trip, land at Natchez or Vicksburg, to deposit their sick and dying, and perchance to bury their dead. They relieve themselves of these encumbrances at Natchez, or at Vicksburg, or some other town on the river.

It would be strange, if a succession of boats, in this condition, arriving every day or two, and generally discharging

their sick and a part of their foreign emigrants with the disease dormant in their systems, should communicate no disease in the crowded and stagnant atmosphere of the river towns. Yet because very often one or two houses are first infected, or because a particular district of a town or city becomes first infected, we find some of the medical faculty, as well as the illiterate, adduce this very fact as an argument in support of the extrication of a pestilent miasm from *some local cause* which they cannot indicate.

In all these instances the disease first manifests itself in those parts of towns and ports where the above species of population mostly congregate, whether near the wharves or not. However they generally find their temporary residence near the wharves and steamboat landings, in some of the numerous crowded wooden tenements of those places.

The number of German and Irish emigrants that arrive at New Orleans every summer is almost incredible; and of late years it is the principal port for which they take shipping, on account of the facilities afforded for reaching the northwestern States on the upper Mississippi. These crowd the deck of every steamboat which leaves New Orleans for the upper country during the summer, but especially when the epidemic begins to rage in the city.

The constant arrival of these northern foreigners in July, August, and September, at New Orleans, and many of them touching on their way at West India ports, contribute materially to aggravate the malignity of the disease in that city, and to extend it into interior towns.

(TO BE CONTINUED.)

ART. II.—*A Case of Thymic Asthma.* By BENJAMIN DENNIS, M. D., of Cincinnati, Ohio.

As there are many highly respectable physicians who deny the existence of thymic asthma, the following case, which was supposed to be one of that kind, and was treated as such, may possess some interest.

In the latter part of December, 1839, I was called to see a child aged 15 months, son of Mr. C——, of Walnut street, who, according to the unsatisfactory history of the case, as gleaned from its mother, had been for the five previous months troubled with difficult respiration, continual wheezing, and occasional attacks of dyspnœa with threatened suffocation. Up to the time just mentioned, the child had been favored with uninterrupted good health; but now, in consequence of the duration of the malady, it was greatly emaciated—reduced almost to a mere skeleton, The attacks of dyspnœa occurred as often as four or five times during twenty-four hours, and each of them created the most fearful alarm concerning its termination.

Having never before witnessed a case of thymic asthma, nor read much relating to its true character, I did not suspect the presence of that affection, but was disposed to regard the case as one of simple asthma. In this error I was not alone, as five other medical gentlemen who had visited the child before I was consulted, had arrived at the same conclusion. With this view the usual remedies applicable in the cure of asthma were prescribed; but perceiving no improvement in my little patient, I suspected that the difficulty might be the consequence of elongated uvula; upon an examination, however, this proved not to be the fact. I then determined upon a consultation.

January 10th, 1840. Visited my patient in consultation with Prof. J. P. Harrison, who, after instituting a careful inquiry into the history of the family, thereby ascertaining that they were predisposed to scrofulous affections, suggested to me

that the case was one of thymic asthma. We immediately proceeded to examine the gland, which was found greatly enlarged, producing a very marked projection of the superior end of the sternum; it extended itself superiorly to the thyroid gland, and laterally to about a half-inch beyond the sternal extremity of each clavicle. The following prescription was agreed upon:

℞ Iodine ℈j.
Hyd. Potassæ, ℈ij.
Aqua distillat. ℥j.
Fiat solutio.

Two drops, to be given in a tea-spoonful of sweetened water, three times a day.

The parts over the gland were directed to be rubbed three or four times a day with salt water.

This course, attended with a gradual improvement in the health of the little patient, was continued for two months, increasing the dose of the iodic solution one drop each successive week, and substituting the ointment of iodine for the salt water.

A small portion of calomel and rhubarb, a grain of each, was prescribed at occasional periods, when the state of the bowels rendered it necessary. Plenty of fresh air and plain, but nutritious diet, were also advised.

The gland continued to diminish in size, and at the expiration of three months from the time of our visit, the health of the little sufferer was apparently completely re-established.

In February last, I was informed through a friend of the family of Mr. C., who is now living in Illinois, that the child had had no return of the affection, and that his health was uniformly good.

May, 1842.

REVIEWS.

Art. III.—*The Practice of Medicine; or a Treatise on Special Pathology and Therapeutics.* By Robley Dunglison, M. D., Professor of the Institutes of Medicine, &c., in Jefferson Medical College, Philadelphia; Lecturer on Clinical Medicine, and attending Physician at the Philadelphia Hospital, &c. &c. In two volumes, pp. 1323. Lea & Blanchard, Philadelphia.

There are few, if any, offices that are rewarded with less kindness and thanks, and few, if any, with more dislike and censure, than that of reviewing—more especially *critical* reviewing. Nor is there any one more certain to be thus rewarded, at least in a higher or lower degree. The reason is plain. If the reviewer be at once both competent and faithful—if he be both able and resolved to do strict justice to the subject of his examination, his conscience, taste, and judgment compel him to find some fault with perhaps every work that issues from the press. And the vanity of letters, the avidity of applause, and the pride of authorship, rarely forgive either censure, exception, or even a neglect to praise.

In truth, if the matter be skilfully scrutinized and correctly judged of, it will be found that there are few tasks which man has to encounter, that are more complicated in character, and more difficult of performance than that of the reviewer. These evils arise, not alone from the matter, the spirit, or the mode of composition of the book he has to examine—nor alone from the united influence of the three. To gain approval, and indeed to escape reproach, the reviewer must, to no small extent, frame his critique in accordance with the tastes, the

judgments, and even with the fancies and prejudices of several classes of individuals.

Of these classes, the author of the work is of course a very important personage. A single hair-breadth of deviation, made by the reviewer, from what he deems the *right line* of respectful regard, or of virtual commendation, is sure to produce silent dissatisfaction, or open complaint.

Are there, within reach of the critique, any number of contemporary and rival authors, who have recently written on the same or on similar topics? If so, their choler is sure to be stirred up, and their ire awakened, by whatever may be said, which does not, in some way, redound to their credit, at least, if it does not minister to their vanity and self-esteem, by some oblique or direct insinuation, that the palm of authorship is adjudged to them. Is any remark made that can be tortured by construction into an imputation towards them of a want of certain excellencies which others possess? That is magnified into an act of condemnation hardly tolerable. Is no form of reference made to them at all? That is wrought up into an evidence of neglect, which is still more offensive. For, to the vain and the jealous, the bitterest potion that ingenuity can compound, or mischief administer, is, *to let them alone.*

Still more captious and difficult to be pleased is a class of readers, who had never spent a previous thought on the subject under discussion, and who are actual strangers to both science and letters. And this class is querulous and presuming in proportion to their ignorance. As respects merit in composition they have no real knowledge, and therefore supply their want of it by noisy pretension. Another description of readers, who give the reviewer neither annoyance nor concern, is the enlightened, the tasteful, and the inquiring, who examine, observe, and deliberate, and ultimately deem it most safe and advisable to judge for themselves.

In the midst of this throng of petty perplexities, (and more might yet be cited,) what is the hapless reviewer to do? The answer is obvious. To thread his way in safety through the entire labyrinth of difficulties that beset him, is impossible. He must either, therefore, in absolute despair, abandon all attempts to free himself from the entanglement created by them; or he must cut the knots which he cannot untie, and resolve to gratify his own taste, conscience, and judgment, by awarding justice to the extent of his ability, and allow each class of read-

ers, whether satisfied or dissatisfied with him, to form such views, and adopt such measures, as may be most agreeable to them. And this is the course which we, on the present occasion, are resolved to pursue.

Toward the attainment of the end he ought to have in view, in the examination of a work on which he means to pass a critical judgment, a reviewer may, as a preliminary measure, do something to prepare himself, by asking and answering the following questions.

1. What were the leading objects of the author in the writing and publication of the work? 2. Are these objects laudable and useful, or the reverse? 3. Does the work contain preparations and means well calculated for their accomplishment?

The work whose title forms the heading of this article, and on a brief examination of which we are about to enter, purports to be a system of the "Practice of Medicine; or a Treatise on Special Pathology and Therapeutics." The true interpretation of this announcement is, that the writer designs to prepare a treatise containing a view of the *whole* "Practice of Medicine," not of any *circumscribed* and *insulated portion* of it.

That this is a title, then, of wide scope and lofty pretension, cannot be denied. Its claim is to empire, or universal authority in medicine, unlimited in extent, except by the limits of medical knowledge, and uncontrolled in power, except by a want of professional means and measures not yet discovered, devised, or reduced to practice. In fact the claim is virtually to a scheme of *medical omniscience*, as respects the past, beginning with the fountain-head, or earliest records of the profession, and reaching without a chasm to the present period. To attempt to write *a system* of medicine (and Professor Dunglison has done no less) is to aim at the pinnacle of medical enterprise. It involves an effort to establish, for the time, a professional autocracy, to continue in force and fashion, until new discoveries and further improvements shall call for a more enlarged and enlightened one. To complete the effort then, is to collect and condense into a single treatise a digest of all that is known of the principles and practice of medicine, at the present period, by the Faculty of the world. An omission of any thing important is so far a failure, and bespeaks in the author either carelessness, neglect, or some sort of incompetency to the task he has undertaken.

The leading object of Dr. Dunglison's "Practice" appears to be to improve the profession of medicine, in four points of view, each of

the four being highly useful and praiseworthy. And these points are its elements as a SCIENTIFIC—A LITERARY—and A PRACTICAL CALLING; in each of which we say, certainly it ought to have been, and we doubt not was the purpose of our author to contribute to its advancement. Nor is this all that his enterprise exacted of him. It was also his duty, as the fourth point, to facilitate, as far as possible, the attainment of an accurate and complete knowledge of medicine, by the younger and less educated portion of the American Faculty—more especially by medical students.

It does not, however, comport with our present intention, to scrutinize very strictly, much less severely, all the fitnesses or unfitnesses of the work for the promotion of these several forms of improvement. An effort to that effect, besides involving a toilsome and unpleasant task, would require for its accomplishment much more time than we have now at command, and a more protracted and profound investigation, than we are willing to encounter. Nor is this all. The documents growing out of it would fill up a larger space than, in this Journal, could be conveniently allowed to it, conformably to the size and economy of the work. Our review, therefore, (if so it may be called,) will consist chiefly of a succinct series of general observations, accompanied perhaps, or rather followed, by a more formal and detailed discussion of one or more controverted points of doctrine.

As already intimated, the object of Professor Dunglison, in the volumes before us, has probably been, (assuredly it ought to have been,) to improve medicine as a *scientific*, a *literary*, and a *practical* profession, and to facilitate the acquisition of a comprehensive knowledge of it by the American Faculty, especially the junior portion of them, and by medical students.

As regards the SCIENCE of medicine, the Professor has made no improvement in it. He has not even attempted to improve it. In reference to that point, therefore, he is not perhaps justly amenable to blame. To find fault with a writer because he has neglected or failed in what he neither has promised nor is prepared to do, would be illiberal and unjust. And such precisely is the case with our author. He is under no pledge, except by implication, to improve medicine in even the simplest of its scientific qualities. Nor, may we judge from aught he has previously done, do we consider him in any degree fitted for a task of the kind.

Whatever may be the value and efficiency of the Professor's mind, as a condensing lens, to collect within a narrow sphere the lights

emitted from a wider one by the original and inventive minds of others, it possesses, if we misunderstand it not, none of the properties of an *electron per se*. It gives not out from itself any of the flashes or coruscations of genius, nor has its course been marked by any of those happy combinations, discoveries, or revelations, which proclaim its possession of creative originality. Though we ourselves do not pronounce it a *positive parasite*, yet, were any other reviewer to do so, we should deem it incautious in an antagonist to enter the lists with him, in a trial of his truth by the ordeal of battle. Indeed might we, without giving offence, institute a comparison on such a subject, we would say that the Professor's mind in its relation to the *minds* of other men, is inferior to the mistletoe in its bearing towards the plant to which it adheres. Though that parasitical product derives from the spot to which it is attached, the matter that nourishes it, yet, by its own powers, it digests, matures, and moulds that matter into its own organism and identity. With the mind of our author, however, the case is different. While it draws its matter of nutriment from the writings of others, it allows that abstracted from each different and dissimilar source, to retain almost entirely its own peculiar form and character. Hence the productions of the Professor, instead of being uniform, consistent, and tasteful compositions, exhibit a crude mosaic or rather patch-work appearance.

We do not deny to Dr. Dunglison either *readiness, hability, or tact* in the *manufacture* (we mean the *compilation*) of books. We acknowledge, on the contrary, that he possesses each in sufficient abundance. But *there* perhaps his book-compounding attributes end. In her allotment to him of the higher and more valuable quality of *judgment*, nature has been sparing, not to say niggardly. Neither are his selections choice, his groupings skilful, nor his arrangements and classifications either able, instructive, or in any way highly appropriate or beneficial. Every thing from his pen, (perhaps we should say *hand*, which may wield other implements besides his pen) is too much encumbered with the minutiæ of detail, and is too scanty in matter of general import. In his nosological catalogues, these faults are broad and striking.

Notwithstanding these remarks, we are compelled to acknowledge, that our author has shown a redeeming share of sound common sense, in his selection of the mode of book-making he pursues. The reason is plain. As already intimated, it is the only mode that he is qualified to practise. Were he to attempt a more elevated and original

one, his failure would be equally signal, certain, and humiliating to him. And as relates to himself, this is one of the decrees of fate, which he has had the discernment and good fortune to discover, and the discretion to acquiesce in, and has therefore been sufficiently successful in his literary enterprises. And had he not thus acquiesced, his fortune as an author would have been, of necessity, the reverse. In reference to this point we shall only add, in corroboration of what has been already alleged, that all the works which our author has hitherto published have been the product of the inferior order of the intellectual faculties—of those which, by observation and perception, receive knowledge, and communicate it by language. Of the fruit and finish of the higher intellectual faculties they are altogether destitute.

In as much, then, as the actual improvement of *medical science* consists in one of the three following forms of action, or in the union of all of them—*the discovery of new matter—new creations produced out of matter already known—or new inferences deduced from one or both, in the capacity of premises;* in as much as this is true, and as Professor Dunglison has fallen far short of such distinguished performances, and even shown himself incapable of them; it follows, as a corollary, that, by the work before us, he has effected no improvement in *scientific medicine.*

On the literary standing of the volumes we are compelled to pass a similar judgment. It is *ordinary* in all parts, except where it is *low.* The style of the work is every where loose, crude, and ungraceful, with frequent outbreaks of pomp and bombast. Without pronouncing it positively obscure and muddy, we are fully justified in saying that it is wanting in that cloudless perspicuity, which, in every kind of composition is the most important quality, and, in elementary treatises in science, is altogether indispensable. Criticism might, *in charity,* deal gently with it, were it the production of a *freshman or a sophomore;* but, coming, as it does, from a grave Professor, in the very meridian of mental life, and who prides himself on being a *British scholar,* (educated, we mean, in England) strict justice should be made to visit it; and, all circumstances considered, that is *condemnation beyond reprieve.* To the simplicity and purity, conciseness and vigor of well prepared composition in the Anglo-Saxon tongue, the style and manner of the work are as utter strangers, as if they were not the product of a native pen. As respects many of the

writer's customary forms of expression, though we do not pronounce them actual *foreignisms*, yet have they more of a foreign than of a *native* cast. They are at best but a hybridous product between the language of Great Britain and that of sometimes one and sometimes another of the nations of continental Europe. In most instances, however, the adulteration comes from France. Yet does the writer bear the character of a ripe and high bred English scholar.

Nor have we yet referred to all the forms of *affected* and vitiated phraseology in which the Professor indulges himself. In instances, which occur so frequently that we venture not to enumerate them, he employs words under a meaning wholly unauthorized by standard lexicographers, and by the most exemplary writers and speakers of the English language.

Among these misused words stands "invoke" a term of whose employment he is so infatuatedly enamored, that he monotonously and in open violation of etymology and taste, judgment, custom, and all other correct guides and authorities of speech, introduces it on almost every practicable occasion. And in carrying out this strange predilection he not unfrequently so distorts and misapplies it, as to render his meaning in the employment of it equivocal. Of this the following exemplification may serve our purpose. "Pathological anatomy—with heedless or too enthusiastic observers—has too often been *invoked* to establish the former of these positions."

A dislocation of a word more awkward and striking than this can hardly be imagined. And very many similar ones, we say, occur in the employment of the same word, in the course of the work.

"*Revulsion*," if we misunderstand him not, the Professor often employs as if synonymous with *revolution*, or entire change, and "*economy*" as if identical in signification with *organism*, *structure*, *or system*. Now every one may and ought to know, and every medical *scholar* does know, that, when applied to the human body, "economy" strictly means the *action*, or *mode* of *action*, and the native habitudes of the *living organism*, or of some portion of it, not the organism or structure itself. Indeed to our taste even the habitual employment of the *word organism* is exceptionable and repulsive. True, the term itself is legitimate English, and may be occasionally used without much impropiety. But several writers of the day, and quite *distinguished* among them is our author, give it a preference and use it with a frequency which we deem pedantic, affected, and tasteless. In

a great majority of cases the word "*organization*" or "system" should receive over it a decided preference. In truth we regard the present fashionable use of it as an unscholar-like dash of *literary dandyism*. Grave Professors should be above a practice so empty and boyish.

True; Professor Dunglison is not, we say, the only medical writer who thus errs in the employment of these terms. Some others still higher perhaps in public confidence and standing, but certainly not lower, have done the same. So much the worse. The larger, stronger, and more authoritative the erring party, the greater is the evil, and the more loudly does it call for correction or removal. Hence we have deemed it our duty, as faithful reviewers, to make it the subject of the preceding strictures. And we regret that a want of room has prevented us from proving, by extracts from our author's "Practice", the truth of the imperfections we have imputed to its style.

Nor have we yet closed our animadversions on what we deem the unscholarlike and sophomorical characteristics of the Professor's style. His predilection for the constant use of a certain set of popular and new-fangled words and phrases, savors not a little of affectation and the unmanly weakness of following the fashion, without calmly reflecting whether it be wise or unwise, right or wrong. This charge we illustrate by specifying the substitution of the words "*normal*" and "*abnormal*" for *natural* or *regular*, and *unnatural* or *irregular*, "*nutritive* or *vital fluid*" for *blood*; "*hyperæmia*," instead of *inflammation* or *congestion* of blood—and scores of others similarly abused, on the exposition of the whole of which we have not leisure at present to dwell. A few more of them, however, we shall here enumerate.

Our author has been guilty of the gross tautological blunder of using the expression "*morbid derangement*," which is tantamount to *morbid morbidity*, or *deranged derangement*.

Another blunder no less discreditable is the substitution of *agency* for *agent*, as in the following sentence.

"If a healthy person exposes his feet to these *agencies*" ("cold" and "moisture"), &c. Now "cold" and "moisture" are not "*agencies*" but *agents*—the former meaning *sorts* or *kinds of action*—the latter the persons or things *that themselves act*. Under no figure of speech, therefore, or construction of language, can either of these words be cor-

rectly substituted for the other. Hence nothing but defective scholarship, or unpardonable neglect, could have produced in our author the blunder here committed.

Again,

"As in the case," says the Professor, "of every other disease, not induced by any mechanical cause, the first impression must be made on the *contractility* or *vital properties* of the part with which a morbific body comes in contact."

This clause is also exceedingly faulty and unscholarlike—if indeed by scholarship, be meant a power or state of preparation to *think*, as well as to *write* and *speak*, with correctness and propriety. The phraseology employed in it is indicative of a striking *inaccuracy* of discrimination and jndgment.

To talk of making an "impression on *contractility*", or any other "*vital property*," is to express an idea (if in reality any idea be expressed by such words), in direct opposition to the principles of physiology, as well as to those of common sense.

A "morbific body" does not, in the production of disease, impress "*a vital property*." As well might our author speak of making an impression on a *shadow* or a *thought*. The thing impressed must be substantive. Through the medium of "contractility" or some other "vital property," the "morbific body" impresses the organized part, produces on it a morbid effect, and in that way changes and deranges also the vital property; and disease is the result. For the organ or part is the *fountain* or *parent-stock*, and the vital property the *issue*. And as is the condition of the former then, so must be that of the latter.

We have said that the Professor's style is at times exceedingly cloudy or even muddy and obscure; and the following sentence proves our assertion.

"It is, indeed, impossible to fix, with any thing like precision, as has been attempted, the exact sequence of the disorder of innervation, circulation, and secretion."

If to this clause any one can attach a single clear, well-defined and determinate idea—one single idea, we mean, more certainly and definitely expressed by the language of it than another—if any one can do this, we shall acknowledge him our superior in *clairvoyance*. We can perceive in the phraseology of the extract nothing but a mass of fog and muddiness.

No matter, we repeat, whether other writers, in common with our

author, employ these words or not. Their habitual use bespeaks affectedness, and an effort to appear at once both learned and modish.

Professor Dunglison should hold himself above such empty ostentation. Men of mind and attainment should and do engage in substantial operations with the machinery of facts, leaving to boys and inferiors in general to play at crambo, and other forms of mere verbal amusement.

These strictures on style we shall close by observing, that our author makes books too rapidly to avoid in them the commission of errors and blunders of every description which haste and a want of circumspection can produce. His aim seems to be *quantity* rather than *quality*—to compile and publish very *many* books, rather than very *valuable ones*. He appears ambitious to be pronounced able, not to compose a *thousand lines*, like the satirist's poetaster, but to make a *thousand extracts*, *"stans uno in pede."*

The rapidity with which the Professor prepares his books is productive of two effects, neither of which is favorable to his authorship. It prevents him from displaying in the style of his works either elegance or taste; and it renders his productions too long. Haste in composition conduces to *superfluity* in words, and *scarcity* of thought. To the condensation of matter and style, much time and labor are indispensable. The truth of this may be illustrated by a well known anecdote of a celebrated sermonizer.

The orator was asked how long a time would be requisite for him to deliver, on a given topic, the best sermon he could prepare by the labor of a week? He answered, *An hour*. How long could you preach on the same subject, after the labor of half a week? Ans. *Two hours*. And how long, if you had no time for preparation at all? Ans. *All day*.

Might we therefore, without giving offence, we would, as friends, advise Professor Dunglison to devote more time to the preparation of his books, and abridge the number of them. By good-naturedly complying he may oblige his readers, by making his productions shorter, and improve his own reputation, as a writer, by making them better.

We strongly suspect that the worthy Professor has allowed himself to be too much flattered by the encomiums passed by certain reviewers, or rather *noticers*, on the prolificness of his mind. He has been highly complimented, on account of having published an unusual number of works, within a given number of years. We beg to as-

sure him that this is a fallacious ground of compliment. True; by hasty book-making he *may* advance his *pecuniary* interest—but never will he, in that way, improve the condition of either his scientific or his literary reputation. We shall only add, that the reviewer who justly censures a writer for his faults, is more truly and beneficently his friend, than the flatterer who applauds him for excellencies which he does not possess. And, without aspiring to the standing of a seer, we venture to predict, that, provided Professor Dunglison receive what we have here said in a proper tone and temper of mind, and make the proper use of it, he will find hereafter more substantial ground to thank us for our animadversions, than he will to thank those who have injudiciously or dishonestly flattered him, for their praises.

That these free and unceremonious strictures on Professor Dunglison's "Practice of Medicine," will be attributed by some persons to unfriendliness of feeling toward him, on our part, is hardly to be doubted. The charge, however, if preferred, will be groundless. We are not unfriendly toward him. But, in the words of Brutus to Cassius, "We do not like his faults." And his style is not only faulty in itself; it is calculated to do mischief by the production of similar faults in others.

With the junior portion of the medical profession in our country, more especially with pupils, the Professor is a popular—I might perhaps say, a *favorite* writer. And favorites are likely to be followed and copied, particularly in the attributes which render them favorites—and in none of these more so, provided they be writers, than in that of style and manner.

It is more than probable, therefore, perhaps even certain, that the style of Professor Dunglison will become a model for imitation by young medical writers, whose taste and judgment are yet immature, and easily vitiated and led astray. And if so, the consequence is plain. Such imitation will operate to the deterioration of the style of the medical writings of our country. Hence, to contribute *our* part toward the prevention of the evil, we have deemed it our duty to pass on the Professor's composition the foregoing strictures. And, should the measure become necessary, or even advisable, we hold ourselves prepared, as already mentioned, to prove their correctness by extracts from the work.

But the most important element of our author's production is yet to be examined. We allude to its practical one. What will be its

bearing on the treatment of disease, the alleviation of human suffering, and the preservation of human life? Will it, in these respects, be productive of any change, amounting to a truly valuable improvement? To be able, under the approbation of our conscience, and the sanction of our judgment, to answer this question affirmatively, would be eminently gratifying to us.

But such gratification it does not fall to our lot to experience. In a practical point of view, the volumes before us present to us nothing with which we were not already familiar, when first we opened them. Indeed, neither on the score of principle nor practice have we been able to discover any thing in them positively new. If to this they furnish an exception, it consists in a catalogue of technical formulæ, to which, by enlightened physicians, possessed of candid, liberal, and independent minds, who rely and act on principles and generals, rather than on petty points and particulars, but little if any importance is attached. To speak more plainly and definitely on the subject, we are unable to discover any solid or even specious ground of belief, that a single disease has yet been, or is likely to be hereafter cured, or a single life saved, through the instrumentality of Dunglison's "Practice of Medicine,,' which might not have been as well, and as certainly effected, had the volumes never been issued from the press. All this, however, would have been excusable in our author, had he, without the addition of any thing of novelty or improvement by himself, in either fact or principle, embodied in his work a correct account of the most efficient and successful modes of practice now pursued in a very large proportional part of our own country. This, however, he has failed to do—for what reason we presume not to know. We refer to the *fact alone;* because in that only do we take any concern. In truth, moreover, we doubt not a little whether he has faithfully recorded the most vigorous and successful forms of practice pursued in any of the sickly parts of our country. Of western and southern regions only, however, do we speak with positiveness.

Instead of examining and recommending, or even treating with common respect, what is well known to be the *most* successful, if not the *only* successful mode of treating the endemic and epidemic complaints of the Mississippi Valley, the Professor has somewhat superciliously, and therefore exceptionably scouted and condemned it. On this point we speak with confidence, and tell him plainly, that the form of practice which he dignifies and honors with the epithet *"philosophical,"*

because he deems it the most enlightened, judicious, and efficient, would, if employed in the treatment of the giant diseases of the West and South, prove, in many cases, virtually an abetment of manslaughter. If it did not itself kill, it would allow disease to do the work. The want of judgment of our author on this subject, and his rejection of, or unacquaintance with, the issue of all experience, are matters of astonishment to us. And that astonishment is increased, by the overweening confidence and utter disregard for the sentiments of others, with which he proclaims and urges his opinions, when contrasted with the limitedness of his opportunities for acquiring the requisite amount of correct information. For, in the knowledge of the diseases of hot climates and valley-situations obtained by observation and experience (and no other sort is of any avail) he is wholly unversed. The Professor is not indeed an advocate for the feeble "gum-water" practice of Broussais, an attempt to introduce which into Philadelphia was recently made by a popular teacher. May we judge, however, from many of his receipts, prescriptions, and general directions, his scheme of therapeutics is, in not a few instances, but little superior.

Without pretending to any *positive* information in the case, we feel authorized to believe, that Professor Dunglison has never been to the south of the 37th or 38th degree of north latitude, and that, even when there, the place of his sojourn was *mountainous*—its elevation, as respects *temperature* and its effects on health, being equal to several degrees of northing. That he should possess then any amount of *self-acquired* knowledge of the complaints of warm and sickly climates, is absolutely impossible. Nor will he himself contend for the contrary opinion. And we repeat, without fear of refutation, that, in relation to successful practice in such places, no other kind of medical knowledge is of any efficiency. Knowledge acquired exclusively from books, unaccompanied by experience, is, for practical purposes, unavailable and useless.

For proof of the correctness of this statement, we need only refer to our author's preface. He there reveals to us the extent and variety of his travels and residences. And they are both considerable. According to his showing, he has been in the north of England, and in Edinburgh and Paris, as a student of medicine, in London as a practitioner, and, as a teacher and a practitioner, in the Universities of Virginia and Maryland, and in the Jefferson Medical College, and one of the hospitals in Philadelphia. And there terminates the career

of his shiftings of residence, and its several concomitants; having reached from about north latitude 54° or 55° to north latitude 37° or 38°, and over a stretch of longitude much greater. But he has never, we repeat, been a resident of a hot and sickly climate. He has never even visited one of the true southern and western States of the Union, and hence knows, expxrimentally and practically, nothing of the character and treatment of their diseases. And he, like all other men, is incompetent to teach, either orally or in writing, what he does not understand. Yet to this truth, which is as indisputable a maxim as any in mathematics, he seems (may we credit his own words) to be an entire stranger.

In the preface to his "Practice" he first tells us that the "great principles of Pathology and Therapeutics are the same every where." And of the truth of that sentiment no ghost need rise from the dead to convince us; in as much as all men fully admit it without such evidence. But all men as fully and firmly reject his opinion, when he immediately adds, that "one who has been well grounded in those principles can exercise his profession with as much satisfaction to himself, and advantage to the sick, in the scorching presidencies of British India, as in the more temperate regions of our own country."

True; a well educated physician can treat successfully the complaints of those burning regions when he has learnt, by a skilfully directed experimental apprenticeship, to do so. But to contend that he can thus treat them, on his first encounter with them, merely because he is "well grounded in the principles of Pathology and Therapeutics" as they are taught in medical schools—to contend for a notion so empty as this, is to set recklessly at defiance, and stubbornly gainsay the sound dictates of common sense, as well as the verdict of observation and experience. Were the fancy of Professor Dunglison on this topic correct, where would be the value of elaborate treatises written exclusively on the diseases of hot climates—or indeed of any particular climates or localities whatever? Such productions would be supererogatory and useless, and therefore mere encumbrances in our libraries. Let young men "ground themselves well" in the mere "principles of Pathology and Therapeutics," and, in our author's opinion, they become on the instant, efficient practitioners. Hence, the worthlessness of the writings of Lind, Cleghorne, Hillary, Hunter, Moseley, Chesholm, and the entire class of writers on tropical complaints!! They are all superseded and rendered stale

and valueless by our author's "Treatise on special Pathology and Therapeutics"!!—because it contains an exposition of the principles of those two branches of medical knowledge! A position more notoriously fallacious than this it is impossible to imagine. Yet is it a fair deduction from the visionary doctrine avowed by our author.

At another of the prefatory passages of the Professor's "Practice of Medicine" we are greatly surprised. It shows, if we mistake not its nature and bearing, a looseness of thought, and a *want* of *comprehension*, or a *miscomprehension* of the correct relation of cause and effect, quite unworthy of its distinguished author. It is as follows.

"Hence the medical officers of our army and navy, especially the latter, whose duties carry them to every part of the globe, are found to be as successful in the management of the cases that fall under their care in distant regions, as they would be in the treatment of those that prevail in the spot where they received their medical education."

To pronounce the statement contained in this extract *a gross exaggeration* would be no doubt deemed language excessively discourteous and exceptionably strong. Yet does the case admit of language still stronger and plainer. The statement involves a *positive misrepresentation*. "The medical officers of our navy" are *not* "as successful in the management of the cases that fall under their care in *distant regions*," as in the treatment of our own domestic diseases, with which they are familiar. The truth of this appears from scenes of distress and desolation that have often occurred in the West Indies, and other intemperate and sickly climates. Our sailors and marines, new to such places, have been attacked, soon after their arrival, by forms of disease equally new to the "medical officers" of the ships. And the issue of the visitation has told a tale exceedingly different from that which our author has inserted in his preface. The mortality among the "new-comers" has been often appalling. Why? Chiefly because the "medical officers" have been unversed in the character and treatment of the prevailing complaint. Yet have they not been altogether uninstructed in the "general principles of Pathology and Therapeutics."

There exist perhaps two principal causes why our sailors and marines are swept off so alarmingly, under the complaints which assail them, soon after their arrival in hot and sickly places. Their constitutions being yet unacclimated renders them more liable to fatal attacks;

and their physicians being in a great measure strangers to their maladies (being certainly inexperienced in the treatment of them), are less skilful in the duty devolved on them. But this state of things becomes gradually amended. Time acclimates the constitutions of the officers and crews; and experience gives skill and tact to the physicians and attendants. And, as the natural consequence of such changes, the disease becomes less formidable and fatal.

All things considered, we are at a loss to conceive how a medical writer could broach and attempt to vindicate a more visionary and untenable notion, than that on which we have briefly animadverted. An effort to prove the existence of an "*elixir vitæ*," or to manufacture such an article, would hardly surpass it in wildness and folly. And in the amount of mischief our author's hypothesis would inevitably produce, could a belief in it be widely extended, it would throw far into the back-ground all the dreams in which the alchymists have indulged. But, fortunately for the credit of medicine, and the interest of man, the glaringness of its fallacy, by preventing its adoption, will extinguish in embryo the evils that would arise from it.

Few things in medicine have surprised us more than the inveterate pertinacity, with which Professor Dunglison, and perhaps some other writers and teachers in the Atlantic States, adhere to their notion, that they are themselves actually competent to the exposition and treatment of the herculean diseases of the West and South, *which they have never witnessed*—and (still more erroneous and assuming) that they can communicate to their pupils a thorough practical knowledge of that which they have never themselves had an opportunity deliberately to study, nor even momentarily to inspect!! The Professor, as we are told, is a sturdy disbeliever in *mesmeric clair-voyance*, and even imputes to those, who are holders and advocates of the opposite belief, a condition of mind scarcely short of *imbecility and foolishness*. Yet does he labor, with the zeal of a fanatic and the perseverance of a martyr, to impress on the world an implied admission of his own *omnivisient* clair-voyance—(may we coin an extraordinary name for so extraordinary a power)—a faculty so clear in its function, and so boundless in its sphere of action in him, that he can, by means of it, while seated in his study in Philadelphia, explore successfully the specialties, as well as the general characters of the complaints of the Mississippi Valley, and learn how to cure them!! "*Risum teneatis amici!!*" So easy and natural is it for gentlemen of a certain organ-

ization, temperament, and caliber, to strain at gnats, as respects the performances of others, and to swallow camels and moose-deer themselves!!

True; in justice to our author, and the other Atlantic writers and teachers, who claim such super-eminent *clair-voyance*, in relation to western and southern complaints, we are compelled to acknowledge that their claim is in *words*, rather than in acts. Hence, when they are making preparation to write, speak, or lecture on a western disease, they consult either western men, or western writings, or both, for the materials they want. Nor can thay possibly derive them of the slightest value from any other source, except by crossing the mountains, and practising their profession for a time in the West. And were they to adopt that expedient, they would soon be convinced of the truth of our remarks—that they must either abandon, in some most material points, their eastern modes of treating disease, or surrender all hope of successful results.

Such, we venture to assure them, has been the case with all pupils, who, educated exclusively in Atlantic schools, have passed the mountains, and pursued their profession in the Mississippi Valley—with all, at least (and their number has not been small) with whom it has fallen to our lot to converse on the subject. They have been compelled to exchange the practice inculcated on them by eastern teachers, for one exceedingly different, derived from western example, observation, and experience.

In truth, to many eastern physicians whom we could name, a sojourn of a few months in the West would be eminently serviceable. The experiment, if properly made, would teach them not a few things of which they have no knowledge, and which notwithstanding they deeply need.

In point of feeling, it would impart to them a more liberal share of modesty than they now possess. Does any one ask us, *in what way* it would thus act? We answer, by teaching them a more accurate knowledge of themselves, *in comparison with others*—ay; even in comparison with *back-woods* men! It would enable them to know, and compel them to confess that, in all new-settled tracts of country, there is no deficiency in the knowledge and fitness of the inhabitants, as relates to any of the essentially requisite pursuits and duties of life. All things necessary to the varying wants and homely comforts of the people are not only done, but satisfactorily done. And this is perhaps as

true of the treatment and cure of diseases, as it is of any other forms of business. Many a practitioner, who travels from house to house through the forest, carrying his powders, pills, and potions in his saddle-bags, could give instructive lessons, and set a profitable example, even to *eastern profesors*, in relation to the treatment of the sick whom he visits. He could convince them that he surpasses *even themselves*, notwithstanding their accumulated and highly and justly-prized stores of *book-knowledge*, as respects the cure of the complaints of the place. Nor to this would Professor Dunglison himself form an exception. In the management of the diseases of the forest, the comparatively unlettered and plain-mannered physician of the "back-settlement" would be superior to the book-learned and polished physician of the metropolis.

Of this superiority the reason is plain. The country practitioner possessed of it is, by long experience, familiar with the diseases he prescribes for, while he of the city, for want of experience, is a stranger to them.

Let it not be imagined, from these remarks, that we, in the slightest degree, undervalue medical literature. Far from it. Our only meaning is, that as regards the cure of disease, it is much less valuable than medical experience. And in this every physician of common sense will heartily concur with us.

Though Professor Dunglison is not alone possessed of the conceit that he is completely accomplished in the knowledge of the nature and treatment of western complaints, and that he can therefore expound them and communicate an accurate acquaintance with them to pupils, as thoroughly and instructively as western teachers; yet, if we mistake him not, he contends for that notion more openly and doggedly, than any other individual in the schools of the east. In truth it has long been our belief, that the Professor has a propensity unusually strong, to prefer a claim to the possession of *unacquired* knowledge—a kind of knowledge, we mean, derived from neither reading, observation, experience, nor oral intercourse, nor indeed from any other source without, or apart from, himself. It would seem to be made up exclusively of *internal, spontaneous*, and *un-labored-for* ideas or fancies, for which he is indebted not even to *himself*, but to a peculiarly bounteous endowment of Nature, which the capricious goddess has bestowed on him, but on nobody else. The following anecdote may serve as a farther illustration of our meaning.

About twenty years ago, (a few years perhaps more or less,) Dr. Dunglison arrived by invitation in the United States, as one of the Professors in the University of Virginia. Soon after that period the writer of this article had the pleasure of an introduction to him, and found him confident in the belief that, though he had scarcely perhaps, at the time of the interview, felt an American pulse, or made a single visit to an American sick room, he had, notwithstanding, a complete knowledge of American diseases, and was perfectly competent to their treatment and cure, and also to become an instructor in them to others.

Nor is this statement made in the form of an inference from a casual conversation. It is the substance of an opinion that was frankly avowed. And although, during the conversation, the Professor himself was perfectly calm, courteous, and self-possessed, one or two of his colleagues deported themselves differently, when the correctness of the belief was delicately questioned. An insinuation quite civilly made, that an English physician *might* be surpassed and even instructed, in his profession, by an American, appeared to be regarded in the light of an indignity. And thus was the conversation suddenly interrupted, or the subject of it changed. Under this head we shall only add, that we have known intimately many European physicians of great respectability, who had migrated from their own country, and become resident practitioners in this. And Professor Dunglison excepted, we are, at this moment, unable to call to mind a single one, who did not frankly confess, that, on his arrival in the New World, he found it *really new* in most things; and that, in particular, he found himself a stranger to the successful treatment of American diseases, until he had become acquainted with it by experience. And, in the nature of things, the acknowledgment could not be other than ingenuous and veritable.

If the foregoing strictures and disquisitions be founded in truth and justice, (and our aim has been conscientiously to that effect,) it necessarily follows that in neither a *scientific*, a *literary*, nor a *practical* point of view, has the profession been improved by the volumes we are examining. Is it then to be hence inferred, that medicine is in no way benefited by the work? We answer, no. A few useful purposes in the profession, the publication creditably subserves. Some of these are the following.

In the first place, it makes a part of the great scheme of *labor-saving* and *power-giving* means and contrivances of the day. It brings

together and presents to its readers, under a succinct and condensed form, a vast amount of medical matter, which no physician could collect for himself, without a most tedious and toilsome examination of detached works; and which, from a want of leisure and books, the great body of physicians in the United States could never collect. By its copious references to authorities, it serves to those who are solicitous of further information on given topics, as an index to libraries to which they may have access. Nor is this all.

It presents to the physicians of the United States a very valuable and praiseworthy example of literary research, ambition, and perseverance. And were that example judiciously analyzed and deciphered, improved and followed out, it might be rendered the most profitable attribute of the book. It furnishes a lesson replete with instruction, as to what may be accomplished by labor alone, without the aid of *genius* or *talents*. For, in the preparation of his system of "Practice," the Professor has not made the slightest manifestation of either. He has shown himself to be, in the strict and entire meaning of the phrase, a "*working man*," possessed of the excellent and essential qualities of assiduty, faithfulness, and perseverance, but, in our view, of little or nothing higher. And the example he has thus put forth ought to prove exceedingly useful and encouraging to all moderately gifted men, who constitute an immense majority of the Caucasian race itself, and the entire mass of all the other races. Even among the Anglo-Saxon and Anglo-American Caucasians, who are in no small degree superior to the other varieties of men, the proportion of the true-highly gifted is extremely small. And even of that number but few employ their talents to the best effect. The reason is plain. To such persons *labor* is irksome and repulsive, and *pleasure* fascinating. They are mostly an intractable compound of opposites and extremes. Hence, trusting to their high natural endowments, for the attainment of their wishes, and the gratification of their ambition, they reject the disagreeable and cling to the pleasing. Hence also they fail of that eminence they might easily attain, and of that usefulness, in whose production they might be greatly instrumental.

By the compilation of his "Practice of Medicine," then, we repeat, Professor Dunglison has done much to gratify and encourage, the great body of moderately gifted men. He has practically shown them, what it is important they should know, that by *industry, zeal,* and *perseverance alone*, without the aid of mental powers above *mediоc-*

rity, much may be done to acquire reputation and reward ambition. For that, in the estimation of a great majority of that portion of the Faculty of our country, who may read his "Practice," his reputation will be enhanced by it, is hardly to be doubted.

In the case before us, therefore, the Professor's example and good fortune will do much in confirmation of the great and important truth, that, where nature, merely by her choice and abundant endowments, confers distinction and its concomitants on *one* man, active energy, enterprise, and perseverance, confer them on thousands. Hence, were this proposition rigidly analyzed, and carried out to its full extent, it would clearly reveal and establish the fact, that men are the makers and marrers of their own fortunes; that they have the control of their own destinies, far beyond what is generally admitted, and that they are therefore to themselves the only FATE to whose influence they are subjected, and whose *fiat* they are positively compelled to obey. So true is this, that those who labor for standing and distinction resolutely and steadily, without faltering or flinching, and under the direction of common sense and sound moral principles, rarely fail in the attainment of their purpose.—One topic more, and our task will be suspended for the present, if not finally closed.

As this article, if read at all, will be read chiefly by the physicians of the West and South, it may probably be expected of us that we should offer an opinion more definite and positive respecting the fitness or unfitness of the volumes before us to subserve, *in particular*, the interests of practical medicine, in the Mississippi Valley.

To this expectation, supposing it to exist, we can reply, *to our own satisfaction*, in a single sentence. Others, of course, notwithstanding all we have said, or can say, will exercise their right to judge for themselves.

After the most strict and impartial examination into the matter and character of the work in question, we are unable to discover in it the shadow of fitness to benefit practical medicine in the Mississippi States. It is a subject of regret to us therefore, that, as honest and conscientious reviewers, we cannot cordially recommend it to western patronage. There yet exists as deep and positive a necessity for an able and efficient work on the nature and treatment of western diseases, as there did before Dunglison's "Practice" was committed to the press; or as there would have done, had the Professor never delivered a lecture nor wielded a pen.

It was our intention to offer a few remarks on one or two points of doctrine maintained by our author in the volumes before us. But the unlooked for length to which this article has already extended, compels us to postpone the completion of our purpose until another occasion.

C. C.

Selections from American and Foreign Journals.

Malformation of the Shoulders.—Last week some observations were made on the general condition of Mr. Nellis, the man without arms, with reference to the power acquired by the toes in the performance of motions that would at first seem impossible to be accomplished by any other means than the fingers. Another opportunity has occurred for a re-examination; and it may possibly subserve the interest of some anatomist or physiologist, if we give the following continuation of last week's notice.

In consequence of being destitute of arms, there not being a single vestige of an arm-bone connected with the shoulder-blades, a peculiar control from early infancy has been acquired over the muscles of the mouth, altogether new and surprising. It reminds one of Lamarck's notion of the manner in which animals obtained their forms—from the operation of the force of desire. The elephant, for example, had a short neck, and having a strong desire to clip the tender grass at his feet or the foliage above his head, kept on desiring from age to age, till finally the upper lip was actually elongated into the now characteristic trunk, dragging the nose in its long train. So it would be, were the doctrine true, in regard to the armless descendants of Mr. Nellis, should there be any; the period would arrive when the lips would take the form of a proboscis!

A new labor devolves upon all the facial muscles, and especially upon those about the forehead. This is observable in holding on the hat, throwing it off, and so on. A spinal disease put a stop to Mr. Nellis's growth in his thirteenth year, and he only measures four feet five inches, and weighs but eighty-four pounds. Yet he is quite strong, especially with the teeth and jaws. A fifty-six is easily raised and thrown some feet, by the teeth, which are large, sound, and well set.

Singular as it may appear, in infancy he managed to creep about as well as other children. There is a peculiarity in his vision, worth the special attention of philosophical visiters. It is this: although the most delicate cuttings in paper are executed by the toes, which necessarily removes the work to a considerable distance from the eyes, his vision at the point of the scissors is minutely distinct. This, like the training of the muscles of the lips and feet, was established by the law of necessity;—nature accommodated the organ to the circumstances of the individual. Objects, such as a book, placed at the usual focal distance, soon produce a painful sensation.

One of the anatomical singularities in Mr. Nellis's organization, is that there are no rudimentary arms. The glenoid cavities are, apparently, completely formed, feeling under the finger as though the ball of the humerus were but just slipped aside. The acromion processes jut out boldly, like a protecting roof, on both sides, and on them, as on two projecting hooks, is hung his coat. All the muscles of the scapula are perfectly developed, and he has such perfect control over them too, that he can throw off a coat or re-adjust a garment to its proper place, about as readily as people who have hands.

These few facts in relation to the actual condition of the unfortunate individual to whom they relate, may be of use hereafter to the physiologist. At all events, it would be quite inexcusable not to notice an anomaly so striking. Mr. Nellis says that he cannot imagine any use for a pair of arms, and therefore manifests no regrets that he is without them.—*Boston Med. and Surg. Journ.*

Treatment of Mesenteric Glandular Affections. By Dr. Charles Clay, Manchester.—Under the name of marasmus, and as an affection of childhood, diseased mesenteric glands are very common, almost daily coming under the notice of medical practitioners, *particularly in manufacturing districts*, where laxity of fibre and much constitutional debility prevail, and where the habit, diet, &c., contribute considerably to its encouragement. But as a disease of puberty or adult growth, comparatively little is known; yet it is certain that a disease strictly analogous to the marasmus of childhood is very prevalent, much more so, indeed, than is generally admitted, and annually carries off many whose deaths are often attributed to very different causes, even in other than manufacturing districts. Three-fourths of the common

cases of atrophy are attributable to glandular obstruction only. From the many cases that I have observed during twenty years practice, and in the treatment of which I have been engaged, a few practical observations with respect to it may not be unacceptable, particularly as there are few cases that call forth more patience from the medical attendant, and certainly none that are productive of greater disappointment, rendering it in the highest degree necessary to make a correct diagnosis, which having been made, a long and steady perseverance in remedial measures is to be pursued. I say a long and steady perseverance, because the nature of the case particularly requires it, not only on the part of the medical attendant, but also of the patient. In all cases of long-continued disease there is an aptness to run from one practitioner to another, vainly seeking that immediate relief which cannot possibly be given. If a correct diagnosis be once formed, and the slightest benefit obtained, every confidence should be placed in the party, and perseverance should be the motto of the invalid.

The treatment of mesenteric affections is so often confounded with that of atrophy from other causes, that great errors have arisen, which it is the more necessary to point out. No gland can be enlarged or indurated by disease without obstructing the operations of nature, for which those glands were especially formed; it is evident, therefore, when such indurations are ascertained to have taken place, our attempts at relief should be directed to the chief cause of mischief. Where glandular obstructions exist, it is evident that the quantity of chyle must be limited in proportion to the extent of diseased structure, and in the same proportion the system suffers from emaciation, not receiving the supply of nutriment sufficient to maintain the system in its former healthy state; consequently, this disease is found in every stage, from the slightest obstruction to the entire obliteration of the glandular action; no chyle poured into the blood, the system is emaciated in the extreme; the limbs or parts most distant from the centre of circulation become œdematous, and death immediately follows.

If, then, glandular obstruction be the diagnosis, what is to be done? This leads me to make a remark or two on some of the general axioms of treatment pursued in such cases, with a view to point out their errors, and substitute a plan that the writer has often pursued with considerable advantage.

Dr. Thomas observes, "That in all cases of atrophy the

patient should make use of food that is nutritive and easy of digestion, and it should be taken frequently, but in small quantities at a time." I fully agree with him as to the kind of food, but maintain that to feed an atrophic patient *frequently* is a very mischievous doctrine, calculated to increase rather than lessen the evil: that the whole of the alimentary canal is much deranged in atrophic cases is certain, and the greatest caution is required in the selection of such food as will not require too great an effort for the digestive function; but for that organ to be continually stimulated by constantly taking food is decidedly injurious, the stomach requiring rest from its usual operations as much as any other organ in the body. The repose of the digestive function is necessary to the well-being of an invalid, and in none more so than in atrophic cases, when it is evident, however much chyme may be formed, no more chyle can pass into the system (in consequence of the indurated glands), if the patient be eating every hour in the day, or if only allowed to indulge three times in twenty-four hours; there can be then no advantage in debilitating digestion by frequent meals, whilst the disadvantages must be apparent to every one on the slightest reflection, viz., it increases general debility, and prevents the due operation of medicine calculated to resolve the existing indurations and consequent obstruction in the mesenteric glands. Impaired digestive function is the consequence, *and not the cause*, of the prevailing disease in this case. The real disease should be primarily attacked with the necessary caution of not impairing the powers of digestion further; it will then be evident that all those symptoms necessarily arising from such causes must give way as the cause which provoked them vanishes. The too frequent exhibition of, and too much dependence on, powerful tonics, appears to me contrary to the diagnosis of mesenteric glandular affections, for it must be useless to waste time in the endeavor to improve the function of the stomach by tonic medicines, which, though considerably impaired, *is capable of digesting more food* than can be converted into chyle. The symptoms of indigestion we so often find as attendants of glandular obstruction, are often increased to a very considerable degree by the ill-advised and constant taking-food system.

Very many cases of this description are treated in respect to the miseries of indigestion, whilst the glandular affection is either totally lost sight of, or treated merely as a secondary matter of minor importance, whereas the very reverse ought to be pursued. The constant stimulus kept up by tonics, in

an emaciated system (extremely sensitive and easily excited), counteracts every effort at reducing glandular induration; and this is rendered still more injurious by wines and other stimulating drinks which have been recommended. Such a mode of treatment might be adopted in the common forms of scrofula with less objection than in mesenteric affection, but I must confess *even in those* I never perceived stimulants attended with any good effect. In atrophic cases, however, they are highly pernicious, although they are esteemed by some as scrofulous affections. When œdema takes place, diuretics have been advised; but as this feature is one pointing out the inevitably fatal termination of the case, such means can only hasten the event, being only another drain on the already debilitated system, without adding anything to the supply.—Should any means adopted prove successful in restoring the glands to a healthy action by resolving the existing induration and restoring them to their original size, then there can be no mistake in the exhibition of tonics, freely adopting every means for restoring the physical powers of the system.

The treatment which I have found most effectual, and which I do not advance on mere theory, but from twenty year's close observation, is the use of a medicine that is generally allowed to be almost a specific in diseases of the glandular system, and that in doses so small as not to excite the disturbance of the digestive organs, combining such means with milder tonics just sufficient to keep the system from sinking any lower, without any anxiety for increasing the physical powers, until the indurated glands may have been restored; under such circumstances I commence by giving the following:—R. Tincture of iodine, gtts. xxx.; Fowler's solution of arsenic, gtts. xxv.; infusion of colombo or gentian, ʒ vi. M. Let one-sixth be taken three times a day.

As it sometimes happens that the solution of arsenic produces pains in the head, I occasionally omit it in the mixture for the space of two or three days, after which it is resumed. By persevering some time steadily with this mixture, I have found the worst cases much ameliorated, and life considerably lengthened, whilst many have been entirely restored to health; but as glandular resolution is of itself an extremely slow process, so it requires both perseverance and confidence on the part of the invalid, and great patience from the medical attendant. It is also necessary in the progress of cure to affect the system very slightly with mercury once or twice, and in some cases of extensive disease of long standing even three times with great advantage, by which means the absorbent vessels

are stimulated into freer action, and the effects of the iodine seem to be improved by it. When it is requisite to give mercury, I prefer affecting the system as rapidly as possible by very small doses of calomel very often repeated, as—R. Calomel, grs. ij.; crumb of bread, enough to make twenty-four pills. Take one every hour until the mouth is affected.

The advantage in this is, that the desired effect is frequently produced within twenty-four hours, when the iodine mixture can be resumed (which it is necessary to omit while the effect is being produced). This plan, if strictly attended to, is one that I can recommend with confidence as a safe and effectual one, applicable to every case of glandular induration, and unsuccessful only in cases too long neglected, where the action of the glands is almost entirely obliterated. The diet should be strictly such as to afford the greatest quantum of nourishment with the least possible exertion of the stomach, to be well masticated, mixed with as little fluid as possible (with the exception of milk), and particularly to avoid those of a highly stimulating character, such as wines, spirits, and fermented liquors: to let a space of at least six hours elapse between taking food, and even then the stomach should not be overloaded. These rules are imperative to the well-being of the patient. Exercise should be of the gentlest description; why horse exercise should be so highly spoken of by many, I cannot conceive; in may instances I have seen it the very reverse of gentle; only fancy a weak, emaciated female tugging at the reins, and urging forward a stupid, rough-paced animal with an exertion highly injurious; unless the adviser would go farther and say the kind of horse he recommends, he might as well send his patient to the treadmill: unless, then, the horse is a very suitable one, I am convinced the patient would progress better without such exercise. Where it can be procured, and weather permitting, an airing in an open carriage, or a gentle walk, is to be preferred; if, on the contrary, the weather is unfit, a swing rocking horse, or exercising chair, are very good substitutes; the mind to be kept cheerful, free from extraordinary excitements, occupied rather on pleasant trifles than on subjects requiring reflection. The atrophic cases of manufacturing districts, however, have but little comfort at command; still I have seen many restored under almost every disadvantage, and am anxious the plan should have a more general application, that its merit may be fully and fairly tested.—*London Lancet.*

Fracture of the Neck of the Femur within the Capsule—Osseous Union.—An example of this is related by WALTER JONES, Esq. in the sixth volume, N. S. of the *Medico-Chirurgical Transactions.* The subject of it was a man upwards of 80 years of age, who, after a fall in Oct. 1838, exhibited all the symptoms of fracture of the neck of the femur. He so far recovered as to be able to move about with a stick; the limb was shortened about an inch and a half, and the foot much everted.

The following remarks by Mr. Stanley are interesting:

"The history of the case is clearly that of fracture of the neck of the femur; the appearances of the bone show that there has been a fracture which has re-united by an osseous medium; and the direction of the fracture is such as, in my opinion, can permit no doubt that it was confined to the portion of the neck of the bone covered by synovial membrane; consequently, that it was wholly within the capsule. The fracture extends through the basis of the head of the bone in the line of its junction with the neck. As in other cases of the same kind, great part of the neck of the bone has disappeared, and, in consequence, the head is proportionately nearer to the trochanter major and shaft of the bone; its re-union has, in fact, taken place in part to the remaining portion of the neck, and in part to the shaft. This union is certainly osseous. In addition to the first maceration of the bone with its surrounding soft parts, it was subsequently immersed for several days in a strong solution of carbonate of potash, and one-half of the bone has been boiled in water for three hours without the slightest yielding perceptible in the line of the fracture.

"The specimen is preserved in the Museum of St. Bartholomew's Hospital."—*Am. Jour. Med. Sci.*

Foreign Body in the Air Passage for Nine Months—Expulsion—Cure.—M. MALSHIEURAT-LAGEMARD communicated to the Academy of Medicine of Paris an interesting case of this character. The subject of it was a woman 58 years of age, vigorous constitution, who when first seen by M. M. had been laboring under a cough for four months. At the commencement of the attack the fits of coughing were violent and accompanied with a sense of suffocation. When she consulted M. M. "the cough recurred every eight or ten days, lasted for one or two minutes, and was unaccompanied with fever or expectoration. The patient could assign no cause for

the attack, nor was any thing learned by auscultating the thorax. The symptoms continued without aggravation, or mitigation, and finally, the patient expectorated along with some mucus, a rough, yellowish calculus, about the size of a filbert nut. On sawing the calculus, it was found to consist of calcareous matter, encrusted on a cherry stone. The patient then recollected that the first attack of cough occurred when she was eating cherries, but if she experienced any sensation of suffocation at the time, it was so slight as to have quite escaped her memory. She remained perfectly well after expulsion of the calculus."

In a case related in the *Ephem. Nat. Cur.* a cherry-stone remained in the air passages for a year, and was then expelled by coughing, encrusted by tartar.—*Am. Journ. Med. Sci.*

Case of Inflamed Hernia.—Prof. Williams exhibited to the Surgical Society of Ireland (Nov. 27, 1841) a very interesting pathological specimen, taken from a patient who had died a short time before in the city of Dublin Hospital, 76 hours after having been operated on for strangulated inguinal hernia. The individual in question, about 60 years of age, had labored under inguinal hernia of the right side for a period of six or seven years. He gave an unsatisfactory account of the antecedents of the case, but distinctly stated, that for the last four or five years he had worn a truss, which, however, retained the hernia very imperfectly, and that he frequently experienced much inconvenience from the hernia descending under, and being compressed by the pad of the truss. On admission to the hospital, he labored under mild symptoms of strangulated hernia, presenting very little indeed of urgency. Being a person of very limited intelligence, it could only be collected, in addition to what has been already stated, that he had been five or six days without having had a fæcal evacuation, during which time the hernia, previously reducible, could not be returned; he had kept his bed three or four days, and had vomited four or five times at long intervals. The hernial tumor was about half the size of the shut hand, it was by no means tense; and, when handled, the patient complained of uneasiness rather than of pain, and that to but a slight degree; there was very little abdominal tenderness on pressure, and the patient was able to lie on his back with the lower extremities stretched out at full length. The pulse was about 90, and

rather small; tongue furred and moist; there was a good deal of thirst; and drink, which was only allowed to be taken in small quantities at a time, did not excite vomiting. The hernia could not be reduced, though, on attempting to do so, a considerable quantity of flatus was obviously expelled from the protruded bowel, and the tumor became more flaccid, and apparently slightly diminished in bulk, circumstances which, coupled with the (almost) absence of pain on pressure, caused the taxis to be persevered in longer than would otherwise have been done. Under these circumstances, small doses of sulphate of magnesia were administered by the mouth, and an enema directed to be thrown up every fourth hour. Two or three small fæcal evacuations occurred during the night, and, in the morning, matters were little altered, except that the patient stated, that he found himself, on the whole, more comfortable. Shortly after the visit, however, he began to vomit whenever he drank, and the tumor became painful, tense, and decidedly tender on pressure. Dr. O'Beirne's tube was now passed up the rectum, but as it failed to give relief, and the symptoms were rapidly growing worse. the operation for strangulated hernia was performed by Mr. Williams, in the evening.

On opening the hernial sac, which contained no fluid, and which was not adherent to the bowel at any part, the finger could be passed all around the neck of the sac, and through the inguinal canal fairly into the abdomen. The inguinal canal was very short, and its obliquity had quite disappeared, as often occurs in old hernia, according to a mechanism too well known to need description. There was, in fact, no stricture to free; no pressure or constriction was exerted by the rings, the inguinal canal, on the sac, or any portion of the intestine. Even now, however, the protruded bowel could not be returned until pressure was exerted on it for, perhaps, a couple of minutes; the obstacle to its return consisting in the condition of the intestine itself, its surface being covered with a thick layer of coagulable lymph, which rendered it rigid and unyielding; it was deemed essential, however, to return the bowel, as it gave the sensation to the touch, (which subsequently proved erroneous,) of containing some indurated mass, supposed to be fæces, in its cavity; shortly after the operation, the bowels were freely moved. The symptoms of peritoneal inflammation, however, continued despite the treatment, which chiefly consisted in the internal and external exhibition of mercury, and the patient died seventy-six hours after the performance of the operation.

On dissection, the coats of the portion of the intestine which had been protruded (the ileum about twelve inches from its termination) were found to be enormously thickened—in some parts fully to the extent of a quarter of an inch. The proper tissues of the bowel were not hypertrophied; the thickening depended on the effusion of coagulable lymph, partly situated on the serous surface of the intestine, partly in the subserous cellular tissue. In consequence of this condition of things, the calibre of the intestine was diminished to less than a fourth of its natural capacity; and it was the deceptive sensation thereby communicated to the touch which had led to the belief that indurated fæces were impacted in the bowels. The serous coat of the intestine was inflamed, and covered at points with gelatinous lymph; but, as manifestly appeared from the inspection of the preparation, the intensity of the inflammation diminished as the distance increased from the portion of intestine that had constituted the hernia. The inflammation had evidently commenced at that point, the morbid alterations being there at their maximum, and had thence extended, becoming less intense as it spread. The parietal layer of the peritoneum was not inflamed. A portion of the abdominal parietes and scrotum, comprising the entire of the sac and of the inguinal canal, had been removed, and were exhibited, whence it clearly appeared that no stricture or compression could have been exerted on the intestine by any of these structures.

Professor Williams said that it might be collected from what he had stated, that he considered the operation had been in this case, to say the least, useless. He nevertheless, gave the case publicity. This had clearly been a case of *inflamed* hernia, and not a case of *strangulated* hernia. But how was this difference to be diagnosed during life? He confessed he did not know. The diagnosis of this distinction, a most important one, had yet to be established. In this case, the point of departure of the inflammation was, beyond all dispute, the protruded portion of the intestine; the preparation placed that beyond doubt, and yet, till within a few hours of the operation, that portion of the intestine was, he might fairly say, almost indolent.—*Am. Jour. Med. Sci.*, from *Dublin Medical Press*, Dec. 29, 1841.

Malgaigne *on Pseudo-Strangulation, or Simple Inflammation of Hernia.*—Strangulation is said to exist whenever a hernia cannot be reduced, and when pain, constipation, vom-

iting, and hiccup are present. Should these symptoms progress rapidly, the strangulation is called inflammatory; if slowly, the strangulation is said to be from obstruction (*par engouement*). In either case, an operation is judged indispensable, so soon as the symptoms have acquired a certain degree of intensity. Boyer says, that we should operate if the symptoms continue to increase; and, in old or weakly subjects, he advises us to operate within the first three or four days.

Here we may ask, did Boyer reflect on the true danger of the operation, when he laid down such rules? Assuredly he did; but, with Pott, he thought "that the operation for strangulated hernia presented, of itself, no danger." In order to determine how far this opinion was founded on fact, I have noted all the operations performed during five years (1836–41) in all the Parisian hospitals, and I find that in 183 cases there occurred no less than 114 deaths. Again, in old people, (from 50 to 80,) on whom Boyer would have us to operate so promptly, I find that 70 out of 97 died. Obstruction, Boyer informs us, is produced by a collection of hardened fæces in the hernial tumor, and this frequently occurs in old hernia. But, we may ask, is this latter a fact? For my own part, I may say, that I have examined more than 3000 cases of hernia, without observing the circumstance on which Boyer insists. At Bicêtre, I have examined two of the most voluminous herniæ on record; one tumor measured 10½ inches in height, and 22 inches in circumference; the other, 8 inches in height, by 23½ in circumference; but in neither was there any trace of fæcal matter. Besides, we can have hard or fæcal matter in the large intestine alone, and the presence of the latter in a hernial sac is not one-twentieth as frequent as that of the small intestine. Hence, in 95 out of 100 cases, this obstruction, such as it is described in books, is materially impossible; it can only occur when the hernia is formed by the large intestine. As regards the latter case, I can affirm that I have never seen such accumulation of fæcal matter in any autopsy that I have made; and, having consulted authors, I find but a single example in which this collection had really taken place.

The cause, then, of this species of strangulation is imaginary; so, I regret to say, are the symptoms which we find detailed in surgical writings. I have myself been misled by the doctrines laid down. I once operated on a case of supposed strangulated hernia from obstruction; there was no obstruction, no strangulation, and my patient died. Similar errors of this kind have been committed by Pott, Dupuytren, and Sir

A. Cooper; and were all the cases published, we should have a long list of them. But is there any way of avoiding such errors? It seems to me, that some few rules of a simple nature may be established.

1. There is simple, non-inflammatory strangulation, which produces gangrene in a few hours.

2. We have simple inflammation, which is of frequent occurrence, and generally limited to the serous lining of the hernia.

3. Finally, inflammation of the whole mass of the tumor; this latter circumstance, however, is generally a consequence of one of the former conditions, and may be excited by the stricture, or by frequent attempts to effect reduction.

Obstruction of the intestine is a mere creation of the fancy; it is inflammation of the serous lining of the hernial tumor which has been mistaken for it. This peritonitis is one of the most frequent complications of hernia, and surgeons have probably overlooked it from not having sufficiently studied hernial tumors in their simple state. It may exist under the forms of adhesive or suppurative inflammation; the adhesive is often slight, and indicated by passing colic, or even by symptoms of indigestion, which sometimes go so far as vomiting and hiccup; the tumor is now irreducible, and the taxis will only aggravate all the symptoms; but rest in the horizontal position, and cold topical applications, will often suffice to effect reduction; in more severe cases, it may require several days or weeks before the inflammation is sufficiently reduced to allow of the return of the intestine. When hernial tumors, in this state, are reduced, nothing generally remains, except some thickening and irregularity of surface at the lower part of the sac; should the tumor not be reduced, then adhesions take place.

The suppurative inflammation is infinitely more rare, and can scarcely be discovered, except after operation or death of the patient. How are we to distinguish this inflammation of the peritoneal membrane from true strangulation, and what should our practice be? The following are the results of my experience on these points.

1. In no case of old and voluminous hernia, where bandages have not been employed, or have been left off for a considerable time, do we find real strangulation; the openings are much larger than the neck of the hernial tumor, and strangulation cannot occur. This is the result of all my observations on the living and dead body.

2. In cases of simple epiplocele, inflammation of the perito-

neal membrane is what is commonly taken for strangulation; I say *commonly*, for I would not deny that strangulation may occur; I have never seen a case, nor have I found an authentic one in surgical works.

3. Hence, in the two cases just mentioned, the operation is contra-indicated; the taxis may be tried at the origin or decline of the inflammation, and the treatment should be purely antiphlogistic.—*Am. Jour. Med. Sci.*, from *Prov. Med. and Surg. Journ.*, Oct. 2, 1841.

On the Stinging Organs of the Medusæ.—The severe effects produced by these animals are generally noticed in all extended works on poisons. Dr. WAGNER, of Gottingen, remarks, that it has not yet been ascertained whether the stinging or burning power of these is to be ascribed to a corrosive liquid or to a mechanical injury. He imagines that his examinations, made at Nice and Villafranca in 1839, will aid in deciding the question.

The origin of the stinging is to be referred to the external surface of the skin. This has a beautiful, brownish, violet and reddish color in various parts; and when separated, which is easily done, there appears the homogeneous, jelly-like substance, which constitutes the real body of the animal. Where the red spots occur, we find, after the skin is detached, round elevations or inequalities like warts.

Between the red grains of pigment are to be observed round balls or bubbles, out of which, frequently, by the aid of a powerful magnifying power, (for this whole organization can only be recognized by the microscope,) fine threads are seen to project. These threads, spirally rolled up, often come out of themselves, but always do so on the application of a slight pressure. The balls or capsules, which contain these threads, are very loosely attached and easily fall, and are rubbed off along with the slime when the medusa loses its skin; they are found in quantity, as are also the threads themselves in what is termed the stinging slime of the medusa, as is easily ascertained when these animals are kept in vessels.

The slightest touch of a medusa causes a perceptible burning sensation, and this is more or less severe, according to the vigor of the animals. They only sting from parts of their bodies where the epidermis is preserved. Dr. Wagner never experienced the sensation when he came in contact with portions in which the epidermis had been removed. In his case, a burning sensation was felt in from a few seconds to a minute

after contact; after a few minutes, a slight redness appeared in his case, and then a simple, lentil-shaped elevation; but, more frequently, three or four near one another. The appearance sometimes resembles nettle-rash. The pain generally soon ceases, but it lasted half a day with one of the party, Dr. Will, and, after eight days, a redness was still perceptible.

The internal substance of the body (jelly) never stings, nor does any part, where the pigment spots, the capsules, and the hairs are wanting.

Dr. Wagner thinks it probable that the stinging has a mechanical and chemical origin, and that the parts above described are poison-organs.—*Amer. Jour. Med. Sci.*, from *Edin. New Philosophical Jour.*, Jan. 1842.

Recovery from Tubular Pregnancy by the artificial production of Abortion.—A woman who had borne three children began to complain in the month of May, 1834, of pains in the lower part of the abdomen on the right side in the situation of the ovary. She soon felt a small tumor in this region, which, from the size of a citron, in six weeks acquired that of a child's head. The pain stretched up the back, and down the right thigh, and became every day more insufferable, so as, at last, to deprive her of sleep. Fever, constipation, slight jaundice, and general dropsy, were gradually induced. She believed herself to be again in the family-way, both from the menstrual secretion having been stopped for eleven weeks, and from feeling those peculiar sensations she had experienced in her former pregnancies. Professor Ritgen discovered that the lower part of the uterus was somewhat enlarged, that the orifice was directed to the left side, and that a tumor was in close contact with the upper part of the uterus, and was movable. After a careful examination of it, both from the vagina and the rectum, but especially from examining it by means of the stethoscope, and hearing distinctly the *bruit de soufflet*, he arrived at the conclusion that it was a tubular pregnancy. He, therefore, with the view of aborting it in its early stage, together with the use of enemas, prescribed first Glauber's salts, and afterwards pills composed of the ergot of rye and extract of aloes, covered the lower part of the abdomen with cataplasms, and put a seton in the neighborhood. Vomiting was first excited, then followed contractions of the uterus and of the tumor; black blood flowed from the vagina, at first spa-

ringly, but afterwards in larger quantity, and mixed with clots and shreds, resembling portions of the fœtal membranes. By the fourth day after this, the tamor was found to have diminished considerably in size, and the pain was greatly abated; but a year and a half elapsed before it completely disappeared. Blood was discharged from the vagina for nearly nine months. She has not again become pregnant.—*Ed. Med. and Surg. Jour.* from *Gaz. Med De Paris*, May 8, 1841.

Tobacco in Hysteria. By Dr. J. H. Thompson, of Salem, N. J.—August 19, 1839.—Frances S., æt. 22, unmarried, dark complexion, black hair, moderate embonpoint, about the ordinary height, after having performed an almost incredible day's work, while her clothes were literally "soaked" with perspiration, sat down, with her bare feet resting upon a cold pavement, and continued in this situation for half an hour or more, until she began to "feel strangely," and as if something was "rising in her throat." She walked into the house and was immediately attacked with violent hysterical convulsions. I saw her at eight o'clock, P. M., about an hour after the attack. She was then upon a bed, surrounded by a number of persons, who, as usual, appeared to think the convulsions could be arrested by *main force*, since their utmost efforts had been directed to the injudicious and unsuccessful attempt of preventing any motion on the part of the patient. Her arms were thrown violently in every direction; her head was forced backward, and with the body formed a complete arch. The muscles of the neck and trunk were under rigid tonic contraction, while those of the lower extremities were not at all affected. The face was swelled; the eyes firmly closed; jaws could be opened with difficulty, but were quickly closed with a loud "snap." Some abortive attempts were made to introduce medicine into her stomach; her feet were placed in stimulating pediluvia, and cold applications were made to the head which was extremely hot. No impression had been produced upon the disease. I tied up her arms and made a free opening into the vein, from which the blood flowed in a large stream. No regard was paid to mere quantity, its effects alone were considered. In a short time the convulsive motions ceased; the face began to lose the dark flush which had overspread it; the muscular system gradually relaxed, and the first intelligible words spoken by the patient since the attack were, "I feel sick." About forty ounces of blood had been abstracted.

20. Patient slept a little last night. Feels this morning, to use her own expression, "as if every bone had been broken and every joint dislocated." Says the catamenia appeared a week ago, and were as usual. Bowels were operated upon by enamenta. Still feels the globus hystericus occasionally in a slight degree. Antispasmodics freely administered during the day. At about the same hour as on the preceding day the patient was again attacked. Assafœtida, ℥ j. with 60 gtts. of tinct. opii, was given in enema, and repeated two or three times without effect. Pulse strong; vein again opened; with the loss of 20 ounces of blood, the convulsions ceased.

These attacks occurred at the same hour the three following days, notwithstanding the liberal use of almost the whole list of antispasmodics, and other remedies, amongst which quinine was given, as the disease appeared to have assumed a periodical character. The convulsions appeared to increase in violence: they lasted for several hours, and left the patient in an extremely exhausted condition. During the attack, her countenance was so altered in appearance and expression that her most intimate friends could not have recognized her. Her throat was the seat of chief distress; desperate and continual efforts were made, as if to tear away something that was chocking her. A distressing "clucking" noise was made, as if the glottis was spasmodically opened and closed. Under these circumstauces I determined to make a trial of the powers of tobacco. On the next attack some leaves were procured. One was placed for a few minutes in hot water, and then spread over the epigastric region of the patient. In fifteen minutes the hysterical symptoms had all disappeared. The patient felt sick and continued so for some time, but did not vomit. At the usual hour on the following day, and also on the day after, she was again seized, but on both occasions the attack was arrested *in limine*, by the tobacco, and returned no more. No other means were employed. The patient slowly returned to her former state of health.

This is but a solitary instance of the use of tobacco in one of the Protean forms of this disease, and I am by no means disposed to place much reliance upon isolated cases. The facts are given as they occurred. It will be for future experience to confirm the efficacy of the remedy, or to reject it as unworthy of confidence in this disease.—*Am. Jour. of Med. Sciences.*

Idiopathic Hydrophobia. By John Kimbell, M. R. C. S. L.—W. K., aged 24 years, of a bilious and lymphatic temperament, has, during the last month, suffered from occasional attacks of palpitation of the heart, occurring generally in the night, and invariably followed by profuse perspiration. On October 4, 1841, he rode a distance of fourteen miles, and, on arriving at the end of his journey, about 12 o'clock A. M., he was seized suddenly with great difficulty of breathing, pain over the region of the heart, and painful sensations over the chest. The paroxysm continued for a few minutes, when the dyspnœa and pain gradually subsided; he afterwards ate a good dinner, and appeared as well as usual, until about eight o'clock in the evening, when all the symptoms returned with greater violence than before, and to so distressing a degree did the dyspnœa increase, that there appeared to be imminent danger of suffocation. He was now bled to eighteen ounces, but without any manifest relief, and the operation was repeated in three hours to the amount of six ounces, which had the effect of considerably relieving the pain. About 5, A. M. October 5, I saw him; he could not speak, although conscious of what was passing around him; I was informed that he had had violent convulsive movements of the arms, which lasted nearly an hour, and he now appeared to be suffering from a spasmodic constriction about the glottis and pharynx, causing extreme difficulty of inspiration, which had a peculiar crowing character; he had likewise a great desire for water, and complained much of thirst; no sooner, however, was this fluid brought into his presence than it was obliged to be withdrawn; the sight of it caused an alarming increase of pain about the larynx with a horrible feeling of suffocation, but with the removal of the water the symptoms became ameliorated.—From so many hydrophobic symptoms being present, I was apprehensive he might have been bitten by a dog, and questioned him upon the subject very closely, but to all my interrogations he shook his head negatively. During the intervals of ease, his pulse was full and soft, and averaged eighty beats in a minute, his tongue was clean; the bowels were regular; and the skin of the natural temperature. Aware that there was predisposition to spinal disease, I examined the back, and found about the lower part of the cervical region, tenderness upon pressure, and I observed that this *pressure invariably* produced an exacerbation in all the symptoms, and of this I fully satisfied myself, and my patient likewise, by repeating the pressure three or four times. A blister was applied over this spot; it rose well, and he soon became able to

swallow. Doses of opium were given by the mouth, and an opium injection was administered *per rectum*. I should have stated that, from the commencement of the attack up to the present period, he has experienced a great difficulty in passing his urine, but none in voiding his fæces. 5th. Much improved in every respect, but when his head was raised the spasm was speedily reproduced. He had a constant smacking of his lips, and frequent twitches in his legs and feet; the right arm found partially paralysed. No headache; no confusion of intellect. 7th. Still improving. Spasms had entirely disappeared; he dould swallow fluids with the greatest ease; tongue clean; bowels well opened; secretions healthy: he can now be raised without suffering. The blister disharged freely. The dorsal region was rubbed with an embrocation containing croton oil, tartar emetic, &c., and quinine was given during the day, with henbane at night. From this period he gradually progressed, and at the end of the month was thought sufficiently improved to resume his avocation. One day, however, previous to his intended departure, he had a return of the dyspnœa, but in a much less degree than before. This was immediately treated with the application of leeches to the cervical region, followed by a blister, when all the symptoms soon vanished. He has two issues, one on each side of the cervical vertebræ, which discharged freely, and he may now be considered convalescent.

THE WESTERN JOURNAL.

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LOUISVILLE, JUNE 1, 1842.

MEDICAL CONVENTION OF OHIO.

The fifth meeting of this body began in Cincinnati, May 16th, at 10 o'clock, in the Hall of the Medical College. About 70 members attended, more than half of whom belonged to the city. Dr. Thompson, of Columbus, was elected President. Dr. Boerstler of Lancaster, late President, opened the sitting, and after his successor took the Chair, delivered an interesting discourse on the ætiological and therapeutic influences of heat and cold. A full narrative of the proceedings of this body, will soon come before the profession in a printed form. We shall, however, select a few matters from our own notes.

A resolution was passed inviting physicians from neighboring States to take seats as honorary members, with the privilege of participating in all the scientific exercises.

It was also resolved that no physician who is the author or vender of a patent medicine or nostrum, should have a seat in the Convention; which sound and salutary restriction we were sorry to see neglected in practice. If our conventions cannot be made up exclusively of

highly scientific men, they should at least include only those who technically belong to that *caste*, and have a degree of professional self-respect that will defend them against palpable empiricism.

While on this subject we may state, that a resolution was passed, censuring the practice now becoming so prevalent with clergymen, of recommending nostrums; and, also, that a memorial to Congress was adopted, praying that the law authorizing patents for medicines may be repealed.

We were happy to observe that a committee was raised to investigate the remote causes and prevention of the *chronic laryngitis* of public speakers, generally but improperly called bronchitis.

Among other resolutions, we may mention one condemning the use of iron chains and manacles, in the treatment, or rather confinement, of insane persons; which it seems is still practised in some of the asylums of the West.

We must not omit to mention, that a resolution, declaring it a professional duty for physicians to promote, both by precept and example, the present glorious reform in the abuse of intoxicating drinks, was unanimously adopted; and we are happy to be able to state, that in most of the hospitalities extended to the members, during the whole session, wine and other intoxicating drinks were not presented.

Dr. Thompson exhibited to the Convention some surgical instruments, which he conceived to be improvements on those previously used. One was a catlin with a concave back, which will apply itself to the bone; another a cataract knife, with the blade set at an angle of a few degrees with the handle; a third was a tourniquet entirely metallic, and designed to avoid venous compression.

Dr. Mussey offered to the notice of the Convention some specimens of fracture of the neck of the thigh bone within the capsular ligament, in which a cure had taken place by ossific union.

A resolution declaring it un-professional to attend families by the year for a stipulated sum, was voted down.

It was by resolution desired, that all the members of the next Convention report what they can from their own observations on the symptoms, pathological anatomy, and treatment of Bilious Fever.

The following request was ordered to be entered on the journal of the Convention:

Snake-bites. Dr. Drake respectfully requests the members of the Convention and the readers of its proceedings, to favor him with

such facts concerning the bites of our venomous snakes, as may have fallen under their own observation, or that of persons qualified to observe. He is especially desirous of learning whether the symptoms produced by the bite of the rattle-snake, the copper head, and the prairie rattle-snake, are the same; whether there is an annual recurrence of any of these symptoms; and to what extent confidence should be placed in the efficacy of those native plants which have been recommended as antidotes.

We have already spoken of the address of the late President, Dr. Boerstler, on leaving the chair; and we will now enumerate the reports and other papers which were read in the Convention. Two of these were by Dr. Harrison, one on the modus operandi of medicines, the other on feyer; Dr. Mussey read a lecture on syphilis; Dr. Worcester on the diagnosis of diseases of the heart; Dr. Dawson a report on trembles and milk-sickness; and Dr. Boerstler, from Dr. Bigelow, a botanical description of some plants to which the former of those diseases has been ascribed; Dr. Thompson made a report in favor of mesmerism, which was accompanied by some mesmeric experiments; and during the discussion which followed, Dr. Drake introduced an account of some experiments recently performed by himself, which will be found in our next number; Dr. Barbee read a paper on the influence of malaria, in giving an intermittent character to certain diseases which do not depend on it; Dr. Wright a paper on certain prejudices against the medical profession; Dr. Hempstead a paper on the climate, topography, and diseases of the lower part of the valley of the Scioto; Dr. Atlee an essey on organic life, and Dr. Gross an experimental paper on wounds of the intestines.

All these papers, except the last, were referred to a committee of five members, to select from them such as they may deem proper for publication. We shall not, therefore, say any thing of them at this time, but await the apppearance of the publication, when we shall notice them according to our views of their originality and value. Dr. Gross' paper, which, by his own request, will not be published in the proceedings of the Convention, presented the general results of upwards of 40 experiments on animals, instituted with a view to ascertain the best kind of suture applicable in wounds of the intestines. Since the reading of the paper, we learn that the author has resumed his researches, and that we may shortly expect a full account of them for the pages of the Journal.

We congratulate our brethren of the western part of Ohio on the success of this, the first session of the convention held in the city of Cincinnati. It continued from Monday morning till Friday evening, two days longer than any of the other conventions; and the greater part of four days was devoted to the reading and discussion of papers. We cannot say that all these papers and discussions were of equal interest, but we are bound to admit, that many of them were both instructive and exciting. They would, however, have been more interesting, had a greater number of members participated in the debate.

To report on a meeting of 70 Doctors, without speaking of harmony, would be a great omission. At one time, the modest and orderly maid seemed in danger of being trampled under foot; but all hearts came to her rescue, and the last two days of the Convention presented as much courtesy and kindness of feeling, as we ever witnessed, in any assembly, even of our *clerical* brethren. It was the serenity and brightness which follow the *clearing up* shower, in whose *falling drops* the bow of better promise was painted in brilliant hues.

With unanimity, and much *good taste*, the Convention adjourned to meet at Lancaster next year, in *strawberry time*.

D.

WESTERN PERIODICALS.

There are now the following medical periodicals in the West:

1. Our own *Western Journal of Medicine and Surgery*, a continuation of the Western Journal of the Medical and Physical Sciences, begun in Cincinnati in the year 1827, and of the Louisville Journal of Medicine and Surgery, begun in 1838—united.

2. The *Western and Southern Medical Recorder*, begun in Lexington, November 1841—not a continuation of the former Transylvania Journal.

3. The *Western Lancet*, begun in Cincinnati, May 1842.

These three journals are published in monthly numbers, the first comprising 80 pages, the second and third each 48, making an aggregate for the year of 2112 pages!

If the West and South should sustain this number of journals, it will speak well for the inquisitiveness and liberality of their physicians. If all who ought to subscribe for and read periodical works

should do so, there would be ample support for the whole of these; for the physicians of the valley of the Mississippi amount to thousands. We have heard the multiplication of journals condemned; but who can prevent it, when and where all have an equal right to engage in the enterprise? As well might we attempt to limit the number of newspapers, steamboats, or medical schools. Wherever we have states united into a nation, there are some enterprises that will be multiplied into high competition; which competition, and it only, will apply the corrective. A journal which cannot support itself will not live very long. Like the Irishman's horse, it will eat up its own head; and whatever loses its head, must die. We know not how many carry fodder to our cotemporaries, but the amount contributed to our supply, although not quite sufficient to boast over, or to satiate a bulimious appetite, is enough for good health; and, while we have none to spare, we shall not quarrel with our neighbors, should they even diminish the quantity. If we do not hypocritically hail their appearance in the common field, we shall practise still greater reserve in attempts to drive them from it. We do not feel bound to praise, but we do feel bound not to dispraise their undertakings. These journals are in the interest of the three medical schools in the West; and may be expected, respectively, to advance those interests by every means consistent with *truth* and a *generous* rivalship. *Such* an emulation will do much good and no harm; and we are quite convinced that no other will be cherished. D.

MEDICAL SCHOOLS OF THE WEST.

Next winter, there will be seven Medical schools in operation in the Valley of the Mississippi and the Lakes. They belong to four States, as follows: In Kentucky—the Medical Department of Transylvania University, and Medical Institute of Louisville; in Ohio, the Medical College of Ohio, and Willoughby University of Lake Erie; in Missouri, the Medical Department of Kemper College and the Medical Department of the University of St. Louis; in New Orleans, the Medical College of Louisiana.

In the Union there are 25 schools, or one for every 682,746 inhabitants; the population of the "Old Thirteen," including Florida, is 10,685,594, and its schools 18 in number; it has, then, one for every

593,644; the new States contain 6,376,972, which divided by seven, gives one school for every 910,996. It is obvious, then, that while seven sounds large for the Valley of the Mississippi and the Lakes, it is decidedly less than eighteen for the old States. To reject fractions we may say, that while the new have one medical institution for every 910 thousand inhabitants, the old States have one for every 593 thousand. Eleven schools in the Valley would not, then, be as large a proportion as now exists in the Atlantic basin. We must confess, that this result has rather taken us by surprise, for we had, to some extent, fallen into the common opinion, that the number of western schools is excessive. And excessive it may be, considered in reference to sound policy, but it is far from being so, compared with the number in the mother States; and as they are nearly stationary, while the daughters are rapidly increasing in population, if retrenchment is to be made any where, it should be among them. We are not prepared, however, to propose retrenchment even there, not being convinced, that the number of schools is too great for the Union, but rather that the number of *their* pupils is too few. *Nota bene* that we have underscored "their," for the entire number of students in the United States is *not* too great. It is only, that too few of them attend lectures. If all who engage in the study of medicine were compelled to graduate, our 25 schools would be filled, for there are at all times even more than 5000 engaged in the study; but instead of that number, not more than 2500 seek the benefits of university instruction. Let the other 1500 come forward to qualify themselves in a proper manner, and our schools will average 200 each—as large a number as they ought to contain. D.

DEATH OF DR. BLYTHE.

Died at South Hanover, Indiana, on the 20th inst. (May), James Blythe, D. D., first professor of Chemistry in the Medical Department of Transylvania University.

Dr. Blythe was a native of North Carolina, whence he emigrated to the State of Kentucky, in early life, leaving it about 10 years ago to accept the presidency of South Hanover College, still keeping himself in sight of the green hills of the State with which he had been so long identified. At the time of his death he was about 76 years of age; and had resigned the presidency several years before.

At an early period of life, almost in youth, Dr. Blythe was ordained as a minister of the Presbyterian church, and continued in that capacity till his death. Many years since he was appointed a professor in the Academical Department of the Transylvania University, in the establishment of which, indeed, he bore a conspicuous and important part. He afterwards resigned, and opened an academy for young ladies in Lexington, still, however, continuing an exemplary and solemn minister of the gospel.

In the year 1816, when the Trustees of Transylvania, under the special guidance of Dr. Dudley and Dr. Richardson, undertook the pioneer enterprise of establishing a Medical school in the West, Dr. Blythe was offered and accepted the chair of Chemistry. The faculty was organized in the autumn of 1817, when, from being one of its members, our acquaintance with the deceased commenced. The class of that, the first medical session ever held in the valley of the Mississippi, consisted of 20 pupils, and in the following March, 1818, a public commencement was held, and the degree of Doctor of Medicine conferred on one candidate. Thus Dr. Blythe's name is subscribed to the first diploma ever granted in the West, a *document* which our brethren of Transylvania should, if possible, have deposited in the archives of their *ancient*, and still prosperous university. The graduate was Dr. Lawson, a native of Lexington. After this commencement, we dissolved our connexion with the department, and, in the following summer, Dr. Blythe also resigned.—In the winter of 1818–19 no session was held, but in the ensuing autumn, two new professors (Drs. Brown and Caldwell) were elected and Dr. Blythe was reinstated. The school then reopened, and in 1823 we were elected a second time, and continued in it through four sessions, during which period we became intimately acquainted with the deceased, for whom we cherished a high personal respect. In 1831, 15 years after his original appointment, he took a final leave of the department, and was succeded by Dr. Yandell of Tennessee; public opinion seeming to require that the professor of Chemistry should be a medical man.

The members of the first faculty, appointed in 1816, were Drs. Dudley, Richardson, Overton, Blythe, and Drake, of whom all are still living except Dr. Blythe. Of the faculty of 1819, Dr. Brown died several years since. At a later period Dr. Eberle died, but as he entered the school in bad health and lectured but a few weeks, he should

scarcely be ranked with its professors; so that we may say that from its foundation a quarter of a century ago, but two of its professors have died. Within that period, the University of Pennsylvania has lost five, and the Medical College of Ohio four, including Dr. Eberle.

Dr. Blythe was not a working chemist and of course had no great skill as a manipulator; but he was a classical scholar and adequately acquainted with the general principles of the science, which he presented to his class in a perspicuous manner. In his moral and social character he was honest, upright, and dignified; proud but not arrogant, firm yet not obstinate—abounding in self respect, but at all times disposed to respect the rights and feelings of others. The largest class which Transylvania ever had, listened to his lectures; and the most flourishing sessions of South Hanover College were under his administration.

But a few weeks before his death, we paid him a visit, and found that from several *slight* attacks of cerebral (apoplectic?) disease, his memory had become somewhat impaired; though he was still able to speak with zest of the early days of our *alma mater*. In former times rather corpulent, we now found him reduced in flesh; and on the whole presenting in both mind and body, a degree of senility disproportionate to his years. Of the immediate circumstances of his death we are ignorant.

Thus has passed away one of the first pioneers of medical education in the valley of the Mississippi. Although 50 years of age when he entered on his duties, he lived to see six other schools organized in that valley, with an aggregate of 750 pupils; a progress which has probably not been surpassed in any other country.

D.

ERRATUM.

In page 363 of the last number, 12th line from the top, in the article on Dr. Bigelow's *Florula Lancastriensis*, after "400" read "genera of."

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